THE
COMPOSITION OF HYDROGEN
AND THE
NON-DECOMPOSITION OF WATER
DEMONSTRATED.

BY

W. F. STEVENSON, ESQ. F.R.S. F.S.A.

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THE
COMPOSITION OF HYDROGEN
AND THE
NON-DECOMPOSITION OF WATER
INCONTROVERTIBLY ESTABLISHED,

Chick

IN ANSWER TO THE AWARD OF A MEDAL
BY THE ROYAL SOCIETY,
WHEREBY
THE CONTRARY DOCTRINES ARE ABSOLUTELY AFFIRMED.
ALSO THE
ABSURDITY OF THE EXISTING SYSTEMS
OF
ELECTRICITY AND MAGNETISM DEMONSTRATED,
AND
THE TRUE ONES GIVEN.

BY

W. F. STEVENSON, ESQ. F.R.S. F.S.A.

> "*Rusticus expectat dum defluat Amnis, at ille*
> *Labitur et labetur in omne volubilis ævum.*—Hor.

SECOND EDITION REVISED WITH IMPORTANT ADDITIONS.

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1849.

DEDICATION.

In the year 1783, Lavoisier announced in his Memoir to the French Academy of Sciences, "that water is not a simple substance, an element properly so called, but that it is capable of decomposition and recomposition."*

Dr. Priestley in the first instance adopted this theory; but some previous as well as subsequent experiments, scrutinized with the mind of a sagacious and reflective philosopher, led him to change his opinion, and in the year 1800 he published a refutation of the doctrine.

This refutation did not convince scientific men, and the doctrine promulgated by Lavoisier has continued to prevail.

That this result is not to be attributed to the *stability* of the theory, but to the absence of that *comprehensive, vigilant, and searching investigation of the experiments* which a theory so important demands, connected with the circumstances of these experiments affording *a double interpretation*, is what I most unequivocally assert.†

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* See this Memoir in Watt's Correspondence, p. 171.

† "This is not the first time," says Sir John Herschel (speaking upon another subject), "by many instances in the

A prejudice seems to prevail that the theory, if assailable, can only be attacked by *additional experiments.* This notion is mischievously erroneous, as it is *assuming* that the experiments upon which the theory is founded have been *ably and fully* investigated, and that a powerful and reflective mind can deduce from them no other conclusion than that which Lavoisier has drawn.

I deny altogether this inference, and maintain that *a revision* of the experiments by a clear, vigorous and unprejudiced mind, will elicit the *double sense* which they convey, and discriminate between the true and *that false conclusion* into which we have been seduced by the *ambiguity* of the experiments; and this I assert *not hypothetically*, but with a *certainty of the result.*

No new experiments are called for—" the daily accumulation of facts," said an able but deceived man, sixty years ago, " will only increase perplexity and confusion."*

This observation was most just. What we wanted

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history of scientific progress, where of two possible causes, each at the moment equally plausible, the wrong has been taken." I however deny that *equal plausibility* attached to the possible causes in Lavoisier's theory.

* Kirwan on Phlogiston, &c. (Introduction.) "Facts multiply and encumber the mind *unless* they are the exponents of a theory or system; without this master spirit, *they merely dissever the material world into fractional parts.*"—Quart. Rev. June 1848, p. 42.

then, and what we require now, is the *sound appreciation* of the experiments which existed when Lavoisier propounded his theory; and the object of this tract is to afford that desideratum, by shewing wherein the experiments have been misunderstood, and what is the true conclusion which they afford.

I have no hesitation in saying that a *re-examination* of the experiments (with the key which I have given for their solution), will leave *no doubt* that in 1783 a false and delusive theory was imposed upon and has continued to be sanctioned by philosophers, whereby chemistry has been perverted, and her analyses rendered fictions.

In the conviction that I have at length arrested the march of an inveterate scientific contagion (which a recent inconsiderate step threatened to perpetuate), established the influence of an important principle in chemical combinations, and thereby infused additional vitality into this branch of science, I dedicate this Tract to the ROYAL SOCIETY, with a deep and profound respect.

29 *Marina, St. Leonard's on Sea,*
24*th February,* 1848.

PREFACE

TO THE

SECOND EDITION.

SINCE the publication of the Second Edition of my first Tract, and the First Edition of my present one, I am not aware that any of our scientific professors have stood up to proclaim the fallacies of the existing systems of Chemistry, Electricity, and Magnetism, and the expediency of resorting to the principles which I have promulgated; and this, notwithstanding, (which I must be allowed confidently to assert) that no scientific man can have perused those Tracts with attention, without arriving at the conviction, that such a step is imperatively called for. Then why this silence in the Professional chair?

A distinguished physiologist has said, "that we meet with such an antagonism at the hands of jealousy, prejudice, and indifference, that truths the most important must be urged again and again ere they make a due impression," and the quiescence which prevails with regard to those which I have published, goes far to confirm the soundness of this remark.

"DUX *regit examen*," and it is possible that our expounders of science are only waiting a signal from their "Magnus Apollo," in which case I will beg leave to suggest to this "naturæ minister et interpres," that his reputation and the interests of society call for the prompt adoption of a straightforward and manly course—no less than that of denouncing the present visionary and effete theories; and that it will be the extreme of folly to prolong a belief in wrecked and hopeless delusions by any disingenuous assumption of there being ground for doubt or delay.

I can readily imagine the disinclination of the "Savants" to abdicate doctrines in which they have been educated—upon which they have written so much, and so *instructively*, and to which they owe their standing in society; but the interests of science cannot yield to those of the individual, and these philosophers must resign themselves to the impending reformation in science, as equally wise men have already done in reforms of another kind; there can be no sliding-scale; reluctance and opposition will avail nothing.

"*Magna quidem, magna est veritas et prevalebit.*" So said our intellectual masters, when genius was in its zenith; when man could produce works at which the highest gifted of the present day "contemplate, and will long continue to contemplate with admiring despair;"* and the philosopher who (Deo

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* See Mr. Macaulay's inaugural speech at the University at Glasgow.

favente) can reach the pure and *indecomposable* stream of truth—must in defiance of all sinister manœuvres, acquire for his works a celebrity that will drive envy from the field, and endure with unfaded lustre to the closing scene of humanity.*

> "Was glänzt is für den augenblick geboren;
> Das aechte bleibt der nachwelt unverloren."
>
> GOETHE.

Sidmouth—Royal York Hotel,
16*th Oct.* 1849.

N.B. I have in a former tract shewn that the existing theories of electricity and magnetism are quite as fallacious as the water theory, viz. that there are no positive and negative fluids, but only one fluid; and that the different phenomena we meet with are the consequence of such fluid being found either in motion *as a current*, or in a stationary or quiescent state.

I have also shewn that the poles of the magnet are merely the issues of the fluid, viz.: its entering upon and leaving a conducting body; and that as the fluid is always moving in the direction of the North Pole, it necessarily turns the needle in the same direction. But as this tract † is not generally known, and the good sense and liberality of our Professors of Philosophy have not yet extended so far as to give it publicity, I shall conclude the present work by giving a summary of such tract.

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* See Sir David Brewster's remark, p. 155.

† Important errors in Chemistry, Electricity, and Magnetism, 2nd edition, 1847, published by Ridgway.

In an article in the "Quarterly Review" of June, 1848 (page 68), the writer (who is in the secrets of Dr. Faraday) says, "Faraday regards the Poles as simply *opening a way* to the Electric current."* Now no mention is made of the source from which Dr. Faraday derived this knowledge,—namely from myself: but from such omission I should possibly not be justified in concluding that, the Doctor intended to assume a discovery to which he had not the slightest pretension, and of which he had not the least notion, until he received a letter from me some years ago, and afterwards found the same doctrine in my first Tract, and which he then treated with *incredulity*.

But who would suppose at a first glance of the passage I have cited, that Dr. Faraday meant to impart (and which it was his intention to do), "that the needle does not point to the North Pole from any attraction between the magnetic or electric fluid and such pole, but simply because the fluid is travelling in that direction, and carries the needle in the same route?" This being the Professor's intention, with what object, or from what motive

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* This and other passages clearly indicate the source from whence the article emanated.

(let me ask) did he disguise the subject by such an ambiguous phrase? Candour and frankness are the characteristics of a true philosopher, and would have well become this popular Professor in the present instance.*

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* See note on THE MORALITY OF PHILOSOPHERS, p. 152.

A PRELIMINARY *EXPOSÉ*

OF THE

DECEPTIONS OF LAVOISIER IN RESPECT OF THE COMPOSITION OF WATER.

This arch-magician so far imposed upon our credulity, as to persuade us that water, the most powerful natural antiphlogistic we possess, is a compound of two gases, one of which surpasses all other substances in its inflammability.

To be sure, he found that upon the explosion (the *union*, as he said, but which does *not* take place) of these gases on forming the water, a phlogistic matter *escaped*, and, seizing upon that fact, he boldly declared that he had "*got rid of phlogiston*;" as if by-the-bye, the separation of one matter from another could justify a chemist in saying, that he had got rid of it, viz. *annihilated* it; and if he did not mean the latter, what was his meaning?

But Lavoisier felt that he had not reached the limit of our compliant and easy nature, and he therefore insisted that this phlogistic matter with its heat and light, had *not* quitted the *known* inflammable matter—the hydrogen—but forsooth, the oxygen; *so that he left the hydrogen, with all its phlogiston and inflammability* to form a component part of the water. Nevertheless, he still persisted in asserting, and PHILOSOPHERS believed too, that he had for ever discarded phlogiston (a matter which he found both mysterious and intractable, and which it would be, therefore, most convenient altogether to dismiss), and this discovery so enchants Lord Brougham, that he exclaims, "MAGNIFICENT FABRIC;"* although it could hardly exceed the scope of an ordinary understanding to see, that in order to *reconstruct the gases* by decomposing the water, Lavoisier must have recourse to this *for ever discarded phlogiston*, and which, having escaped, could necessarily *not* be found in the water.

But Lavoisier had not yet trespassed far enough upon the domain of our intelligence, and he ventured therefore, to make a farther advance, and at the same time that he admitted that a phlogistic matter had escaped from one or other of the two gases, he roundly and broadly asserted that neither of them was a compound body, but that *both were elements.* He did not, however, miscalculate the calibre of our credulity. He knew well its elasticity; for we not only believed this impossible doctrine, but deliberately adopted it, and it now forms a part of our chemical code.†

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* See his Lordship's Life of Lavoisier. In this "Magnificent fabric" Lord Brougham includes Lavoisier's other untenable theory of combustion.

† Even the atomic theory of Dalton is built upon the assumption that hydrogen is an element.

Even this was not enough for this mischievous and satiric chemist, and he therefore concluded with a masterpiece of effrontery; and after having proclaimed that he had got rid of the igneous principle "Phlogiston,"—that he had eliminated it as a chemical agent—he calls the water, "The RESERVOIR OF COMBUSTIBLES."*

Now, when he had decreed the extinction of Phlogiston, for what purpose could he have constructed this phrase, but as a finishing stroke of irony, and *ne plus ultra* of mockery and burlesque?

But the most crafty piece of finesse, and which carries with it an indisputable *intention* to mislead, was his insisting that the phlogistic matter which escapes on the explosion of the gases, *comes from the oxygen*, without there being a shadow of ground for assuming that oxygen is a combustible matter; but which assertion proceeding from so celebrated a chemist as himself, he knew would necessarily prevent an examination of the *hydrogen*, a correct knowledge of which, as he foresaw, would at once demolish his theory.

After this successful essay on the *Condescendence* of the human race, what protection—what guarantee, let me ask, have we against the general adoption of any theory, however absurd and mischievous, be it scientific, moral, or political, which any distinguished *homme d'esprit* may think fit to intrude upon the world, if he have influence enough to insure the consent of his compeers, or power enough to silence their opposition ?*

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* See Lavoisier's "Memoire" to the French Institute in Watt's Correspondence and Lord Brougham's Life of Lavoisier.

The mischief which this theory (originated in 1783) has caused to chemical science is incalculable, and the unfortunate sanction gratuitously given to it by a body so distinguished as the Royal Society, must necessarily prolong the evil, by disinclining scientific men from perusing any work in which the correctness of the theory is questioned.

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* It will be said that this possibility shews how invaluable must be the public press. True, it increases the difficulty of imposture. But how often do we find the columns of a leading journal forestalled upon a particular subject, and in which case the chances of deception *are immeasurably increased.*

THE
COMPOSITION OF HYDROGEN
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DISTINCTLY PROVED.

> " *Wherever obsequious reverence is substituted for bold inquiry,* TRUTH, *if she is not already at hand, will never be attained.*"—HALLAM'S HISTORY OF EUROPE during the Middle Ages.

" IRON consists of an earth *sui generis*, united with a certain quantity of PHLOGISTON, and a certain quantity of heat."

" ALL METALS *agree in that point.* The difference is in their earths, which according to their nature absorb more or less phlogiston."

" *The more of phlogiston a metal contains, the more heat is likewise found in it.*"

" That Gold is a compound of an earth *sui generis*, and of phlogiston, probably nobody will call in question; and that gold is not soluble in acids *unless it has parted with its phlogiston*, is likewise known and confirmed by the most decisive experiments."*

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* Scheele on Air and Fire, translated by Dr. Forster.—Scheele in his first proposition does not seem to hold heat to be a property of phlogiston (though that seems to follow from his third axiom) and which arose, no doubt, from his having found that phlogiston, though forming part of the metal, did not constantly give out

The above sentences, in giving the indisputable definition of a metal, supply the TALISMAN which is to dissolve the ILLUSION of the decomposition of water, and restore to Science one of her most valuable elements.*

I have already observed, in the second edition of my former pamphlet,† that being impressed with the vast *improbability* that a fluid so important in the

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sensible heat. It is only when the phlogiston quits the metal with rapidity that the heat is apparent. But it is clear that there can be no heat when phlogiston is absent. Dr. Black therefore called this heat in metals *latent*. See the subject treated more fully, *posteà*.

Mr. James Watt, says Dr. Black, made it appear probable that metals owe their *malleability and ductility* to *a quantity of latent heat combined with them.* Watt's Corresp. p. 154.

Dr. Black, says Dr. Thomas Thomson, shews that softness and ductility of metals are due to caloric. Thomson's Elements of Chemistry, 5th Edit., vol. i. p. 94.

"Bergman found by a careful analysis of iron, that cast iron contained the least *phlogiston,* steel more, and *wrought iron the most of all;* for the HYDROGEN GAS was at that time considered an indication *of phlogiston contained in the metal.*"—Thomson's Chemistry, vol. i. p. 377.

* *This Talisman was most graciously imparted to the author by the* GENIUS OF CHEMISTRY, *who after an absence of thirteen lustres has returned to our Planet, and in compliment to Great Britain, first alighted on the English shore.*

† See "Most important Errors in Chemistry, Electricity, and Magnetism," wherein are clearly explained the CAUSES of the POLARITY OF THE NEEDLE, and of the ELECTRIC PHENOMENA. 2nd Edit. published by Ridgway, 1847.

constitution and uses of the globe we inhabit, as WATER, instead of being a simple element should be a compound of *gaseous* substances, (a fact which if substantiated would be at variance with those attributes which the order and economy of the universe compel us to connect with the idea of that INFINITE POWER whence all things derive their existence) I was led to examine the experiments upon which this theory is grounded, when the result assured me, as I had anticipated, that it was altogether a FALLACY.

A similar reason to that which induced me to examine the theory of the decomposition of water, raised an equal doubt in my mind as to the accuracy of the doctrine of the ELECTRIC FLUID; a doctrine by which *two distinct and opposite properties are given to this matter,* necessitating the conclusion that it must be composed of two distinct substances, without the possibility of pointing out, why, or with what object they are either conjoined, or separated: the not least extraordinary part of the case being, that these two opposite properties are met with *simultaneously in every atom of the fluid.*

Now this theory of Electricity appeared to me as exceptionable as that of the decomposition of water, and an examination of the experiments upon which it is based, satisfied me that it was quite as indefensible; and which I established in the tract to which I have already referred.

In the same tract I also demonstrated that the magnetic needle does not owe its polarity to any attracting power, but simply to the circumstance of the magnetic fluid passing over our globe in a northerly direction, and causing the needle, a conductor of the fluid, to point in that direction.*

It seems indeed extraordinary, that it should not sooner have occurred to philosophers that some deception was likely to be masked by appearances which led to conclusions so strikingly inconsistent and improbable as those which flow from the present theories of Electricity and Magnetism.

My desire was therefore to point out the fallacies which have thus most lamentably contaminated the three branches of Science, Chemistry, Electricity, and Magnetism, but more particularly Chemistry, which from the adoption of false principles and consequent erroneous analyses, instead of progression, has been for more than half a century in a state of retrocession.

I had entertained the hope that what I had already written would have induced at least *a re-examination* of these theories ; but two events have occurred since the publication of my pamphlet which have shaken my faith in that expectation, and caused me to resume my observations.

The one event is the publication of the "Correspondence of the late Mr. James Watt on the discovery of the theory of the decomposition of water," edited by Mr. Muirhead,* and which has been expressly brought forward to establish Mr. Watt's priority of claim to the merit of having stated that water is composed of two gases, hydrogen and oxygen; and the other and most momentous event, is the fact of a *golden medal* having been recently conferred by the Royal Society† upon Mr. Grove, a late member of the Council, for a paper (amongst others) by which that gentleman is said to have proved that water can be decomposed by "*heat,*" which necessarily means "*heat alone,*" as otherwise the paper would be a deception.

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* See what Dr. Faraday says upon it in the note to the "Dedication," *anted,* p. 9.

I, however, confidently anticipate that the present revised edition will effectually remove all prejudice and doubt from the minds of professional men of science, and at once dispose them to reconstruct the existing systems of Chemistry, Electricity and Magnetism, and to place them upon natural, probable, and philosophical bases.

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* See "The Correspondence of the late James Watt on his discovery of the Theory of the Composition of Water, by J. P. Muirhead, Esq., 1847." As in the Appendix to this work is most conveniently given the transcript of the different memoirs of Meusnier, Lavoisier, and Monge to the French Academy of Sciences upon the decomposition of water, and also M. Arago's *Eloge* of Mr. Watt, with Lord Brougham's historical note, together with the papers of Mr. Cavendish and Mr. Watt upon the same subject, it contains every thing necessary to be known, except, indeed, the infinitely more important information of the experiments of Priestley, Scheele, and Kirwan, and which it was the policy of the propounder of the new theory and of the French philosophers to pass over, and if possible to bury in oblivion.

† November, 1847.

With respect to Mr. Watt's claim to priority, assuming, as I shall do for the purpose, that the discovery is not (as it really is) a phantom, but has a real foundation, I intend before the conclusion of these pages, to shew, that neither Mr. Watt nor Mr. Cavendish have any claim to such priority, and that the merit, if merit be due, attaches exclusively to the French chemist, Lavoisier.

This I propose to do with the sole view of confirming, what I think will be shortly made to appear, that although the opinions of celebrated men are entitled to much respect, we must never implicitly rely upon such opinions in difficult and knotty cases; philosophers, mathematicians, and legists, with all their "means and appliances to boot," occasionally taking as erroneous views of a subject, and proving as fallible as men of minor note.

It is, however, not to be disputed, that if we begin our investigations with *correct data*, an acquaintance with mathematical rules, and the acquirement of habit, will enable us to arrive at the solution of various problems with much facility and precision.

It is, at the same time, equally indisputable that if, unfortunately, we commence with *incorrect* or false data, the same system of inductive reasoning only serves to plunge us more rapidly into further and greater error; and as every deduction we may then make will only be a wider divergence from truth, it is clear that we must continue to descend from error to error, until we arrive at the *ne plus ultra* of obscurity; and such appears to me to be the present state of chemical science.

It is to be much regretted that mathematicians have not been able to discover that grand *desideratum*, "the ascending system," or the means of arriving at the sources of light and truth. Here we are however thrown upon our own naked resources; upon the original powers of the mind; or rather upon that power whose comparative perfection constitutes intellectual superiority—the power of *accurate perception;* that faculty which enables us to see wherein one thing differs from another;* and the happy and lucid expression of which perception constitutes what we so much admire, eloquence.†—(See note on the study of mathematics, p. 136.)

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* "Knowledge is nothing but *the perception* of the agreement or disagreement of any two ideas."—Locke's Life by Lord King, vol. ii. p. 275.

† The advantages of a clear perception and a facility of expression (and when we find the one the other is seldom I believe wanting) forcibly occurred to me on meeting with a passage in the 2nd edition of Dr. Thomas Thomson's admirable work on "Heat and Electricity," purporting to be extracted from the Philosophical Transactions of 1838. This passage is upon that *very important* subject—not a bubble of hydrogen—but an "*Electric Spark.*" It seems that among other discoveries of our men of science, it has been found that an electric spark is capable of undergoing different modifications, ranged under the different heads, of a "*Glow,*" a "*Brush,*" &c. &c., and which makes it essential that we should have *a clear definition* of the

But to return to Chemistry.

The Royal Medal conferred upon Mr. Grove, on the ground (as one of his claims to distinction) of his having proved that water can be decomposed by "heat," is most certainly, "*an untoward event*," and one which when we consider the effect which such a distinction, awarded by so learned a body, is likely to have upon the scientific world, may prove greatly injurious to the cause of Chemistry, by checking all further inquiry upon this grave question.

The deliberate adoption of this proposition by the Royal Society* ought to create considerable aston-

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spark itself in its simple state. That desideratum therefore is furnished as follows:—

"*The spark is a discharge or lowering of the polarized inductive state of many dialectric particles, by a particular action of a few of the particles occupying a very small and limited space.*"

Dr. Thomson, with great humility, adds, that he has given *the definition* in the *author's own language—the ipsissima verba,* as he did not *clearly* understand them.—See Dr. Thomas Thomson on "Heat and Electricity," 2nd edit. p. 100.

I will venture here humbly to ask whether the SCIENCE, which to be communicated requires such *an elucidation* as that I have just quoted, might *not* have been more appropriately deposited any where else than in the Philosophical Transactions of the Royal Society? It is perhaps not surprising that minds thus embarrassed and offuscated with the daily augmentation of experiments little examined and less understood, should have been puzzled and confused with the Water Theory.

* It is very far from my wish and intention to cast the least disrespect upon the PRESIDENT and COUNCIL of the ROYAL SOCIETY. Every body must know that the appreciation of a

ishment, since it has always been understood, and is now received as an admitted and established fact, that *neither* the heat of the *strongest furnace*, nor the still greater heat produced by a *burning lens*, could effect the decomposition of water.*

This inefficacy of heat, after a careful examina-

paper upon a special subject, such as Chemistry, must *finally* depend upon the opinions and report of those gentlemen who are *presumed* to be specially acquainted with the subject. My observations must therefore be understood to apply exclusively to the Committee of Chemistry, and to no other portion of the Royal Society. Of the names of the gentlemen composing this committee I am at this moment ignorant, and I have no motive whatever to induce me to make the inquiry.

I must, however, remark, that such a total absence of Information (a fact which will be elicited from a perusal of these pages) in a conclave of scientific men upon a subject on which they were *voluntarily* about to pronounce, and actually did pronounce, a most *important* opinion (an erroneous one, as may be supposed) pregnant with vast consequences is, I believe and trust, without a parallel in the annals of science.

* See Elements of Chemistry, by Dr. Thomas Thomson, 5th edit. p. 104. The only possible apology for the paper of Professor Grove's appearing in the Transactions of the Royal Society, is that its Author and the Chemists who recommended its adoption, were wholly unacquainted with the writings and experiments of Mr. Cavendish with regard to "inflammable air" or hydrogen, (of which, as we know, he was the discoverer) and with those of Priestley and Watt upon the same subject; as with such knowledge, such a paper could not have been written; and without it no paper on the Water Theory could merit a moment's consideration. I say this in the absence of all intention to be uncivil, and as the result of a candid opinion, which will not be disputed by any chemist who will take the trouble of investigating the subject.

tion of the experiments, seems to me so *clear* and *indisputable* that, notwithstanding the conclusion at which the Royal Society has arrived, I must *confidently* assert that heat alone will *not* decompose water, and that to produce those gases, from whose appearance it has been inferred that water has been decomposed, *a metallic substance or some electric agent is indispensable*, and that such metallic substance produced the result in Mr. Grove's experiment, which is by him and the Royal Society attributed to heat alone.

Indeed upon this point all difference of opinion must or ought long since to have ceased; for it is *in substance* the subject of a memorable note of Mr. Cavendish's, annexed to his last and celebrated paper upon the decomposition of water. "I *know*," says Cavendish, "of *no experiment which shews inflammable air* (Hydrogen) *to be pure* PHLOGISTON *rather than an union of it with water*, unless it be Dr. Priestley's experiment *of expelling inflammable air from iron by heat alone.* I am not sufficiently acquainted with the circumstances of that experiment to argue with certainty about it; *but I think it most likely that the inflammable air was formed by the union of the* PHLOGISTON *of the iron-filings with the* WATER *dispersed among them, or contained in the vessel in which it was heated, as iron seems not to be disposed to part with its phlogiston by heat alone* without being assisted by the air or some other substance." (Phil. Trans. Abr., vol. xv. p. 492.)

It is clear then, that Cavendish to the latest period of his life believed *hydrogen to be a compound of phlogiston and water, and that the phlogiston was derived from the metal.* Such also was the opinion of Watt. "It is my opinion," says Watt, "that *inflammable air contains a small quantity of water and much elementary* heat." (Same vol. of Phil. Trans., p. 555.)

The above note of Mr. Cavendish's may, perhaps, require a little explanation to shew how it bears upon the point in question.

Preliminary, however, to the consideration of the note of Cavendish, it will be as well to make a few remarks upon "Phlogiston."

I beg therefore to say that in adopting this word, which is found in the quotations from Scheele at the beginning of this tract, I have no intention of reviving an old dispute, that has, *in substance*, long since subsided. There is no magic in words when their signification is perfectly understood, and I therefore profess to use the word "phlogiston" solely from its appearing to me to be the best adapted to express *the inflammable principle, or electric matter*,* and in order to *contra-distinguish* that principle from hydrogen, which Lavoisier calls "*le principe inflammable aqueux*," but which is unquestionably a compound of the electric matter and water.

By this expression of Lavoisier, and from the rest of his paper, it is clearly to be collected that he means to say that his inflammable principle (hydrogen) is *derived from the water as one of its constituents*, and *not* that it is the inflammable principle *combined with water*, as in that case hydrogen would cease to be a principle. In the use of the word phlogiston therefore in these pages I merely mean "*the inflammable principle.*"

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* Or the igneous base of that matter, should the Electric fluid ever be discovered to be a compound.

That there exists such a principle in combustible bodies, is as clear as the sun when it shines on a bright summer's day. Lavoisier grounds his theory of the decomposition of water, upon this inflammable principle being one of the ingredients of water: his arguments constantly turning upon the necessity of this principle existing everywhere, even in the vegetable creation, (where he also finds this very hydrogen) it being, as he says, furnished by the water.

If therefore there should be floating in the minds of philosophers a notion that Lavoisier *had really discarded altogether phlogiston* this error cannot be too soon corrected; as he in fact *merely got rid of the name*, and not of the matter which he called erroneously, instead of phlogiston "hydrogen," and which latter only contains the phlogiston; and insisted that the source of this inflammable matter was to be found *exclusively in water*, and that by the decomposition of that fluid it was furnished to all the other substances with which it was found to be united.*

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* Dr. Thomas Thomson says with regard to this getting rid of phlogiston, "This did not overturn the hypothesis of Mr.

That this phlogiston or inflammable principle exists in metals is *incontrovertible,* as upon heating a calx or oxide of iron in hydrogen in a closed vessel, the *gas disappears,* the inflammable principle having been visibly absorbed by the calx *which is thereby restored to its metallic state,* while *water and oxygen* are the only other matters found in the vessel.

The late Mr. James Watt, whom his compatriots, aided by M. Arago, are now desirous of fixing upon as the prior claimant to the notable discovery of the decomposition of water, acknowledges this restitution of the metal by hydrogen. "It was reasonable," he says, "to *conclude* that *inflammable air must be pure phlogiston, or the matter which reduced the calces to metals.*"*

I cannot, however, allow this remark of Mr. James Watt, to pass without observing, that it would indeed have been reasonable, (as he says,) so to conclude, *if* water had *not* been found in the vessels after the reduction of the metal; but very unreasonable *after* that fact was known, as the hydrogen which upon Lavoisier's theory would have been wanted to form the water, must have been absorbed by the metallic calx.

Kirwan, who made phlogiston and hydrogen synonymous, because there could be no doubt that such a substance as hydrogen exists, *but hydrogen, if it be phlogiston, must be a component part of every combustible body.*"—See Dr. Thos. Thomson's Chemistry, vol. 1. p. 140.

* See Watt's Correspondence, Introductory Remarks, p. 34.

Mr. Cavendish, it will be remembered, first brought hydrogen (inflammable air) into notice. Hydrogen, I say, that *unquestionably compound body*, which from having been erroneously taken for an element, has made chemistry a chaos of false analyses.

This same matter (hydrogen), *Dr. Priestley also took for the true phlogiston*, as he thought that he had forced or separated it from the metal (iron) *by heat alone;* but Mr. Cavendish was so satisfied, from the accuracy of his own experiments, that *the heat had only assisted* another agent (water) in obtaining the hydrogen, that he recorded that opinion in the very important note which I have before cited.

What, then, let me ask, was the result of this difference of opinion between these two philosophers? Why this, that Dr. Priestley became convinced of the greater accuracy of Mr. Cavendish, and admitted in his subsequent writings that Mr. Cavendish was right, and that water had combined with the phlogiston to form the "inflammable air" or hydrogen.

Thus, then, were two material facts established and placed beyond controversy; viz.: 1st. That hydrogen could be produced by *a metal, water, and the heat* of an ordinary fire. 2ndly. That *heat and a metal alone* would *not* generate hydrogen.*

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* "There seems the utmost reason," says Cavendish again, "to think that *inflammable air* (hydrogen) was, as I have before said, *phlogisticated water*, or pure phlogiston, *but in all probability the former*."—*Watt's Correspondence*, p. 136. And Mr.

Now we see from this that the possibility of producing hydrogen from water *by heat* without the aid of a metallic substance or some electric matter, *is wholly out of the question;* and it is also in direct contradiction with all the experiments of Stahl, Scheele, Priestley, Cavendish, Lavoisier, Monge, and every other experimenter, contemporary or since, that I have met with.

If, indeed, Mr. Cavendish's mind had not been dazzled and unsettled with the vision of a purely fanciful theory, and *electricity and the nature of the voltaic pile* had been at that time well understood, the fact recorded in his note, (the last production of his pen, I believe, in the Philosophical Transactions upon the subject of hydrogen or inflammable air), must have convinced that penetrating and sagacious philosopher what a BUBBLE was the notion of water being decomposable.

Heat, then, of the *intensities* I have stated (viz. the strongest furnace and the largest burning-lens), having failed to decompose water—a fact which has been established by the most skilful chemists, from the most convincing experiments, and is, therefore, adopted by the ablest writers, including Doctor

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Watt says, in his letter to Mr. Hamilton, of 22nd April, 1783, (found in page 20 of the Correspondence) that "all substances which produce inflammable air, are substances which *contain some water firmly united to them, &c.*" This, be it remarked, is a very different thing from water containing the inflammable matter.

Black and Dr. Thomas Thomson—what, let me ask, then, was the *novel* and *extraordinary* heat employed by Mr. Grove? Why, he heated a piece of thin metallic wire, which he connected with the water.*

Now can it be believed that Mr. Grove, and the Chemist-Philosophers of the Royal Society, selected to examine his experiment, (though unacquainted with the experiments of Cavendish and Priestley), should never have *suspected* that some *fallacy* lurked in the process, and that with the heat *there existed some other agent* which had escaped their notice, or whose instrumentality had not been properly appreciated?

Yes, *that unnoticed agent was a small metallic wire, a little piece of metal, which communicated with the water*, and which, with the assistance of *a white heat*, produced all those MARVELLOUS BUBBLES, from which the decomposition of water was inferred. The heat, therefore, instead of being the primary cause, merely acted a *secondary* part, that of exciting the metal to part with its electricity, and which by the aid of the Oxygen in the water it did do.

Now, will it be credited that "the Committee of Chemistry of the Royal Society" chose to adopt the *futile and idle notion* (I hope to be pardoned for expressing thus strongly my impressions) that *heat*

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* See p. 48.

alone had produced the hydrogen and oxygen gases from the water, in *direct opposition* to all the authorities upon the subject, including Dr. Black and Dr. Thomas Thomson; and that, too, notwithstanding I had distinctly stated in page 25 of my former tract* (which was then lying upon the table of the Royal Society), that Mr. Grove, in the experiment for which the golden medal was *afterwards* awarded to him,† had *overlooked the circumstance, that electricity had been communicated to the water by his heated wire?*‡

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* See this Tract mentioned in the Note, p. 6, *anted.*—The unfortunate publication of Mr. Grove's paper in the Philosophical Transactions will, it may be expected, make the Royal Society more circumspect with regard to the papers of professional members of their Council and other influential Fellows, who may be supposed to be rather anxious about medals, on account of the celebrity they confer: nor should this vigilance be relaxed in deference to any University degree, as albeit the Divinities of Cam and Isis have the power to make Masters of Arts, and even Doctors, it is JUPITER TONANS alone who can make the Philosopher.

† The paper on "Heat" was one of the papers for which the medal was given.

‡ Lavoisier produced hydrogen copiously by *dropping water into a gun barrel raised to a white heat.*

Dr. Priestley, in his "Refutation," page 6, says, that he procured *more inflammable air from iron by steam only, than by its solution in any acid.* Again, in page 23, he says, *Zinc made red hot, and steam passed over it, gives out hydrogen freely.* The quantity of Electricity which is given out by steam from a metallic boiler, may be seen daily at the Polytechnic Institution.

The same inflammable air (Priestley says again, page 18) as

It is Lord Chancellor Bacon, I think, who, when alluding to the philosophers of his time, said that "everything must be brought to the nature of

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that produced from iron, *is obtained by passing steam over red hot charcoal.*

It will be *clearly established* in this Tract, that every metal contains a phlogistic matter *identic* with the electric fluid, or the phlogistic base of that fluid, if it be not an element. *Consequently, whether water or steam be acted upon by a metal, or by the electric fluid,* the process is substantially the same, the *heat* applied in such experiments being merely *auxiliary* to the liberation of the electricity, which, uniting with a small quantity of the water, forms the inflammable gas which has been *mistaken* for a decomposition of the water.

M. Pouillet, of the French Institute, has most strangely fallen into the same mistake with Mr. Grove. He finds that electricity is generated by evaporation; but says, that a *chemical change must take place in the water before electricity can be produced;* for that in evaporating *pure distilled water in a platinum basin,* no electric phenomena are observable; but if however small a quantity of *salt, or acid, be added to the water,* electricity will be generated.—(See "The Pharmaceutical Times," vol. 3. p. 301.)

He, therefore, like Mr. Grove, and the "Committee of Chemistry of the Royal Society," has no idea that the salt, or acid, acts upon the metal, and thus liberates the electricity. Let M. Pouillet try the salt and water, or the acid and water, in a *glass* or *china basin,* and *then* (if he can) *demonstrate the electricity.*

Is it not most extraordinary that so clever a man as M. Pouillet, with this striking evidence before him, should not *at once* have traced the source of the electricity to the metal?—Why if the heat which a small piece of metallic wire can contain, were sufficient (as the Royal Society have pronounced) to decompose water, the Tropics would not be habitable, and many of our hot springs could not exist.

tinder or gunpowder, ready for a *spark* to set it on fire, before they discover it." Now I had even exceeded this precaution by actually furnishing Mr. Professor Grove and the Committee of Chemistry with the spark—the *electric spark itself—and yet it was of no avail.*

That excellent "Committee of Chemistry" considered themselves too well informed and experienced to listen to any admonition which conflicted with the tenets of their great master Lavoisier. That great man had effectually relieved Chemistry from the phantom Phlogiston—he had got rid of that intruder. So they thought—but how rashly, unadvisedly, and calamitously the prejudice against phlogiston, or the igneous principle, has been raised and fostered, these pages will assuredly disclose.

The "decomposition of water" has been for many years the *creed* of chemists, and it has been taken for granted that *in adopting* that theory, *phlogiston is totally discarded;* and this convenient doctrine has been assumed *upon the declaration of Lavoisier.* All the contest, therefore, of late years, has not turned upon the investigation of the grounds upon which Lavoisier established his theory, (that was too clear to admit of a doubt), but simply to decide whether he or Mr. Cavendish has the best claim to the discovery.

The *great and important topic of phlogiston*, which was the grand subject of discussion with Stahl, Scheele, Priestley, and contemporary philosophers, has therefore passed *sub silentio*, and our present chemists *are as wholly unacquainted with the subject* as if it had never existed ; and phlogiston is a word which it has been long decided to banish *as worthless* from the chemical vocabulary.

I say, therefore, deliberately, and in the adoption of Mr. Grove's paper, " *habemus confitentem reum*," that the "Committee of Chemistry" is *totally unacquainted with the subject.* I am consequently quite sure, that it will create very great surprise to those gentlemen to be told, that though wits have exhausted their brains in discussing the claims of Mr. Cavendish, Mr. Watt, and Lavoisier, to the invention of a theory which has caused more mischief than the rape of Helen and the siege of Troy, that such theory *does not get rid of phlogiston*, and that Cavendish and Watt *never agreed with Lavoisier upon that part of his doctrine* ; that, so far from it, Mr. Watt, in all his correspondence, and in his papers published in the " Transactions of the Royal Society," *always and invariably* said, " that 'inflammable air' consisted of *water and a phlogistic matter* ;" and he *also invariably treated* Cavendish and Dr. Priestley's experiments of producing hydrogen by iron and water *as decisive that a metal was essential in the production of that gas* ; the words constantly employed by Watt being "*phlogisticated* air," " *dephlogisticated* air," and "*phlogisticated* water ;" and that, with respect to Mr. Cavendish, he not only uses *uniformly* the same expressions, but in the conclusion of his famous paper* he says, "*As the commonly received opinion of* PHLOGISTON *explains all the phenomena at least as well as M. Lavoisier's,* I HAVE ADHERED TO THAT." So that up to the conclusion of the lives of these two eminent philosophers we find them entertain the belief *of the existence of a phlogistic principle.* Now this intelligence will, I am confident, come *wholly by surprise* upon the "Committee of Chemistry."

My doctrine, therefore, of hydrogen being a *compound of a phlogistic principle and water,* instead of being an element of water, is merely *the repetition* of an opinion always entertained (as far as the existence of a phlogistic principle is concerned) by Cavendish, Watt, and Priestley, with this marked distinction, that all those philosophers were incapable of supporting their opinion, from *not being able to shew this phlogistic principle uncombined with water;* whereas I advance a step farther, and say, ay, *and prove* it, too, that this principle is the electric fluid,† and that it is this same phlogistic principle which, combined with water, forms hydrogen; and by so doing, the whole fabric of the decomposition of water dissolves before our eyes, *and bids adieu for ever to the printed columns of science.*

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* See Philosophical Transactions for 1784, p. 153, to be found in Watt's Correspondence, p. 111.

† Or the phlogistic base of that fluid, if the latter be not an elementary substance.

In order to penetrate and comprehend this most important, *and evidently little understood matter of hydrogen* which is the *key* to the solution of that terrible enigma, the "decomposition of water," let us look into the facts, and see how the case stands.

Metals contain a phlogistic matter (Cavendish and Watt state the fact in their papers inserted in the Transactions of the Royal Society *not merely once, but repeatedly*), a fact upon which there can be no difference of opinion, as it is proved by the electricity evolved by a mere juxta-position of contrary metals—by the aid of acids upon metals—*by the operation of the voltaic pile—by the revival of a metallic calx or oxide by electric sparks*, &c.* If, however, there should be any doubt upon the subject in the mind of the reader, such doubt will vanish before he has finished with these pages.

The question then is simply reduced to this: *is the phlogistic matter found in metals and other combustible bodies, hydrogen, or not?*

Dr. Priestley, we have seen, thought at first it was, as he imagined that he had procured this phlogistic matter *in that shape* (*i. e.* hydrogen) from the metal by a simple heat. If he had done so, electricity and hydrogen would, as a necessary consequence, have been identified—they would have been, clearly, one and the same matter. But Mr. Cavendish proved that *water must come in contact with the metal to produce hydrogen,* and that heat merely stimulated its action. Hydrogen, then, must necessarily be the *phlogistic or electric matter chemically combined with water by the agency of heat.*

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* When two different metals, viz. zinc and silver, are placed the first above the tongue and the other under, you have immediately an electric sensation, which arises from the zinc being partially oxidated by the saliva, when it parts with a portion of its electric matter, and which passes through the tongue in consequence of the under metal or silver acting as a conductor, and being less easily oxidated than the zinc.

I do most sincerely and earnestly entreat our scientific chemists to confine their minds to *the thorough investigation of hydrogen,* that "*crux philosophiæ,*" before they embarrass themselves with that complex question, the "decomposition of water." But to return to the subject.

It may however be said, that if electricity be what the metal yields, glass and other substances contain this electric matter, and ought therefore to do the same thing. This however is an erroneous view of the subject; glass will retain *on its surface* a certain quantity of electric fluid, but in that respect it acts as a mere non-conductor; it *contains, as part of its substance, no such fluid* as in the case of metals;* and the evidence is, that it will yield no hydrogen with the aid of water and heat, nor through the agency of acids; in fact, it is, as chemists well know, *an in-*

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* Dr. Black shewed that the ductility of metals was due to caloric; and thus, if the phlogiston or electric matter could be combined with the substance of glass, it would become *malleable.* —See note, p. 15.

combustible body ;[*] whereas the phlogistic or electric matter is found to be *combined* exclusively with *combustible* substances, as the name imports. I am aware that this remark will be considered superfluous to most readers, and I only profess to have made it in answer to cavillers, and *ex abundanti cautelâ.*

Such, then, being the definition and nature of hydrogen, it follows that it does *not*, when deflagrated with oxygen, *generate* water, as its name would signify, *but merely renders up or restores the water with which the phlogistic matter was combined, or which it held in solution.*

Can any chemist examine *diligently* and *attentively* the experiments where hydrogen is produced, and not see that the operation is such as I have stated it to be ?

It is, then, *this substance which demands the scrutinizing eye of the philosopher,* as it is the pivot upon which the great question about water turns ; *for it explains at once why it is indifferent whether you act upon water by a* METAL *or by the* ELECTRIC FLUID, *when you want to produce hydrogen.*

If it be said that the metal, in forming hydrogen with water and heat *may* give out *something else* than this phlogistic matter or electricity, (though Lavoisier distinctly asserts that it parts with nothing,) I answer, *that every analysis of metals proves that a metal is a compound of two substances only*, viz. *a metallic earth, sui generis, and a phlogistic matter.* It is upon this phlogistic matter's quitting the earth, that the latter unites with the oxygen and thereby forms what is called a metallic oxide.*

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[*] *The definition of incombustible bodies is this.*—Incombustible bodies are *neither capable of undergoing combustion, nor of supporting it.* An incombustible body heated to redness *cools afterwards, and is unchanged.* Thomson's System of Chemistry, 5th Edition, vol. i.

* With regard to a phlogistic matter being one of the constituents of a metal, we have the evidence of *Lavoisier himself.* He says, "M. Meusnier had several times *revived* the calx of lead, with *powder of charcoal in closed vessels,* and that *he and M. Meusnier had ascertained what quantity of "inflammable air" charcoal contains,"* and which Lavoisier afterwards furnishes.—*Watt's Correspondence,* p. 187.

Lavoisier says, farther on, "that the calces of metals *are revived in inflammable air only partially,* which he attributes to the calces *not parting sufficiently with their oxygen.*" This observation is correct, as the phlogistic principle will only enter the calx when the oxygen has quitted it, as does the oxygen which enters only when the phlogiston has left it; *but they never unite or are found united in or with the same substance; a circumstance worthy of particular note,* and which I shall have occasion to comment upon, when I say a few words upon combustion.—*Ibid.* p. 191.

Lavoisier, again, notices the revival of minium by inflammable air, extracted from VEGETABLES by distillation; thus shewing the universality of this phlogistic principle.—*Ibid.* p. 192.

Dr. Priestley ascertained what quantity of hydrogen could be produced by different kinds of iron, and he found wrought iron produced the most, and that 120 grains of wrought or malleable iron yielded 96 ounce measures of hydrogen.—*Dr. Priestley's "Refutation,"* p. 61. Dr. Black also shewed that softness and ductility were due to caloric or "latent heat."—See note *anted,* p. 16.

It is again, proved, that this *oxygen may be driven off, or removed from the calx or oxide, by a stream of electricity*, when the phlogistic matter will resume its former place, (that which the oxygen had just occupied), *and thereby reconstruct the metal: and which is then found restored to its primitive lustre and other properties.*

This revivification of the metallic earth into a metal by electricity, was first pointed out by Father Beccaria, and as it is an experiment of vast importance to the understanding of the subject I am discussing, I shall (although it may probably be well-known) give it in a note below.*

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* On looking over the Catalogue of the Library of the Royal Society, I did not find this work of the Padre Beccaria; I however found the experiment in the "Encyclopédie Méthodique," and also in Singer's work on Electricity, and from which last work I have extracted it. Other similar experiments are also given in the *Ency. Metropol.* "*Mixed Sciences,*" vol. ii. p. 112.

"Introduce some oxyde of tin into a glass tube, so that when the tube is placed horizontally the oxide may cover about half an inch of its lower internal surface. Place the tube on the table of the universal discharger, and introduce the pointed wires into its opposite ends, *that the portion of oxide may lay between them.* Pass several charges in succession through the tube, replacing the oxide in its situation, should it be dispersed. If the charges are sufficiently powerful, a part of the tube will be seen stained with *metallic tin, which has been revived by the action of the transmitted electricity.*" M. Singer also gives the following experiment: "Spread a few drops of a solution of silver upon a pane of glass, and place upon it a small piece of *platina wire,* and a similar piece of *copper wire.* Upon bringing the wire into contact with each other, the voltaic current is formed, and the result is a

Many other and perhaps more convincing experiments of the same kind are to be found in Priestley's works, and also in those of Scheele and Kirwan; and more lately in Sir H. Davy's; but the fact of metallic earths being revived by electricity is, I apprehend, now generally admitted.

The *identity of the electric fluid with the phlogistic principle in hydrogen*, is further established (if any doubt could be entertained upon the subject), by the fact, that to *restore* a metallic calx or oxide to its condition of a metal, it is *indifferent* whether you pass through it a stream of electricity, or *heat the calx in* hydrogen, or in dried charcoal powder,* all these being the well known experiments of Dr. Priestley, Meusnier, and Monge. But to return to the paper of Mr. Grove. It must be now manifest that the opinion pronounced by the Royal Society, that "*heat*" will decompose water, is the result of a misapprehension the most extraordinary, perhaps, that has ever emanated from any learned or scientific body.

Why, the experiment of Mr. Grove is a mere echo or repetition of that of Dr. Priestley's made sixty years ago, for obtaining hydrogen by heating

beautiful vegetation of *metallic silver,* which surrounds the platina wire." "Other metallic oxides may be revived in the same way."—*Singer's Elements of Electricity,* pp. 187, 370.

* "Lavoisier and Laplace, assisted by Volta, found that strong electricity was generated by burning charcoal, and that by increasing the quantity, a spark could have been easily obtained." —*Ency. Britan.* 7th edit. vol. 8., pp. 599 and 606.

iron filings in a closed vessel, and which occasioned Mr. Cavendish's remarkable note, before cited. The principle of the two processes is precisely the same; the difference is only in degree.

Dr. Priestley employed a *large quantity of metal and very little water*, and, with the addition of heat, obtained a *considerable quantity* of hydrogen; while Mr. Grove uses a small quantity of metal in proportion to the water employed, and obtains some bubbles only of hydrogen, and necessarily some few of oxygen, but of which gas Dr. Priestley took no account, as he was then wholly occupied about hydrogen.

Why, Lavoisier tells us, in his Memoir of 1783, that two students of the *Ecoles des Mines* "had discovered, *that red hot iron immersed in water under a glass, gave out inflammable air.*"*

Now, will Mr. Grove, or the "Committee of Chemistry," have the goodness to state what distinction they would draw between immersing *platina* raised to a *white heat* in water (Mr. Grove's process), and the *red hot iron* which the students had introduced into the same fluid?†

Is there one iota of difference between the two operations?

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* See Watt's Correspondence, p. 180.

† Will they say that platina and iron differ in this, that platina contains no phlogistic matter? why, it is matter of notoriety that the *purest metals* are those which contain the greatest quantity of caloric or *latent heat*, malleability and ductility depending entirely upon the quantity of such heat.—See notes p. 16, *antea*.

Interested, however, as was Lavoisier to bolster up an improbable theory, he was by far too wise and experienced a chemist to draw from this experiment so wild and childish a conclusion as that *heat alone* had decomposed the water. That notable discovery was reserved for the Royal Society, at the close of the memorable year of 1847.*

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* Surely, before deciding a question of such vast magnitude as this, the "Committee of Chemistry" ought, *with a due regard to the reputation of the* ROYAL SOCIETY, to have made itself a little *acquainted* with the subject. It ought surely to have known, that besides the papers of Cavendish and Priestley, the printed Transactions of the Society for the year 1782 (p. 195), contained a most important paper of Mr. Kirwan's, (a gentleman, I hardly need to say, of great sagacity, and most eminent as a chemist), and of which paper I will give Mr. Kirwan's explanation, as it is found in his "Essay on Phlogiston and the Constitution of Acids," published in 1787, p. 102 :—

"In my opinion," says Mr. Kirwan, "*metallic substances by calcination lose their phlogiston* ;" and, after some other observations, he adds, "to substantiate this opinion it is necessary to prove that phlogiston, or inflammable air, in a concrete form, exists in metallic bodies endowed with their metallic splendour and peculiar coherence. *I have proved this* (Phil. Trans. for 1782, p. 195) by shewing, 1st. That many metals during their solution in acids produce inflammable air. 2ndly. That metal calces are reduced (brought back) to metals by merely heating them in inflammable air, which they visibly absorb. 3rdly. That inflammable air may be expelled from them, *in vacuo,* by mere heat and moisture. 4thly. That *imperfect* metals are never restored to their perfect metallic state but by substances containing the inflammable principle."

In the evident absence of all knowledge of the experiments of

The results of all these processes perfectly accord; and must, I should hope, after what I have stated, be now fully understood. They all produce hydrogen, but in different quantities.

Beccaria—(it is hardly necessary to say that this is not the author fo the work on "Crimes and Punishments")—of Bergman, Stahl, Beccher, Scheele, Cavendish, and Priestley, if the Committee had merely glanced at the Society's printed Transactions *about* the period of Lavoisier, it would have saved itself from the deplorable error into which it has fallen, and the Royal Society from the discredit which such an error must entail upon it.

It may here, however, be very *plausibly* asked, if all this had been proved by Mr. Kirwan, how did it happen that he abandoned his theory and, with his contemporaries, allowed M. Lavoisier's to supersede it? To me this is inexplicable upon any other ground than *marvellous dulness of apprehension—an obtuseness of the perceptive powers*, incompatible with our notions of the *celebrity* of the philosophers of those days. The ancients would have said, "*Fata obstant.*"

All the opponents of Lavoisier's theory *believed* that the inflammable air, or as he called it, "*le principe inflammable aqueux,*" was a compound of an inflammable principle and some other matter; and they must all have known of Beccaria's experiment, which reduced the calces of metals by electric sparks; *i. e.* restored their reguline or metallic form; and yet they could not draw from thence the *unavoidable inference*. that the inflammable principle in the hydrogen and the electric sparks must be one and the same matter. This plain and unavoidable conclusion they certainly did *not* arrive at; and being *in vain* pressed for their phlogistic principle *separate* from the inflammable air, which they *admitted* their incompetency to establish, they allowed Lavoisier to walk over the course. But as to being *reconciled* to his theory, I believe this *never to have been the case;* nor has it ever remained free from *strong objections*, as may be

I appeal to the scientific chemists of the Royal Society, whether, in principle, there is a shade of difference between any of these processes?

It may indeed be said, but it would be a miserable shift, a lamentable "*naufragii tabula,*" that Mr. Grove used a platina wire in his experiment, which it is difficult to oxidate. But is not platina a metal? *Does it not contain a large quantity of phlogistic matter?* Nay, it is so combustible, that Lavoisier, by a charcoal heat, with oxygen, found it to burn with *all the brilliancy of an iron wire;* and

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collected on reading Dr. Thomson's comments upon it, in the first volume of his "Elements of Chemistry," 5th edition.

When Lavoisier was pressed upon the subject of the *revival of metallic calces or oxides as well by charcoal* as by the "inflammable air," all the answer he gave was, "that *the charcoal attracted the oxygenous principle from the calces by its superior affinity.*" But when the calces lost the oxygen, what gave *them,* I ask, *the inflammable principle* which restored *the metallic lustre, &c.?* It is evident that he could give no answer, except the fallacious one, that the calces are still metals when deprived of their oxygen. This objection alone, without one additional argument, destroys his theory.

A great parade was made in France about Mr. Kirwan's Essay, which was translated into French, and divided into portions which were distributed among the different sections of the French Academy of Sciences, and were, as it was said and supposed, severally refuted by Lavoisier, Fourcroy, Morveau, and Monge, but who, in fact, contented themselves with the admission that the phlogistic matter was not producible, and insisted that the decomposition of water was so satisfactorily proved, that there was an end of the question.

Dr. Van-Merum reduced it into an earth by means of a burning lens.

It is also proved that *platina as well as gold will lose the property of combustion by exposure to the atmosphere*;[*] and it is well known in chemistry, that the metals which part with their phlogiston with difficulty, *are accelerated in that operation by external or ordinary heat*, and which platina absorbs more than any other metal.

What doubt, then, can exist for one moment, that heat and water applied to platina will deprive it of some of its phlogiston? and as to its containing that matter in abundance that I have already proved.[†]

Why the humidity of the atmosphere is enough for that purpose, as we have already seen.

Dr. Pearson, in his paper printed in the "Philosophical Transactions," suggests that *gold and platina* may contribute to form inflammable air.[‡]

The fact, however, is not to be disputed, that Mr. Grove makes use of a *metallic wire* heated to a white heat to produce this hydrogen; and we have seen that *without a metal* heat would be employed upon water in vain.

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[*] This was proved by Thenard and Duloy. *See* "Thomson's Chemistry," vol. i. 5th edit. p. 274.

[†] See note p. 42.

[‡] See "Philosophical Transactions" for 1797, p. 142; and see Priestley's "Refutation," p. 55.

Upon what ground, I now ask, had the Royal Society a right to say, that "heat" alone will decompose water?

Am I not, then, justified in asserting (which I do with all due respect to the *general* scientific attainments of the gentlemen to whom the observation may refer), that *neither* the "Committee of Chemistry" who reported upon Mr. Grove's paper upon water, as entitling the author to the honour of a medal, nor the Council who conferred it *were sufficiently masters of the subject* upon which it treated, to pronounce a definite opinion upon the merits of that paper; and that by having incautiously so done, they have solemnly sanctioned the doctrine that water can be decomposed by *heat alone*; whereas *not only heat, but a metal, was employed in Mr. Grove's experiment*; and I repeat again, *take away this piece of metal, and the water will bid defiance to a* UNIVERSE OF HEAT!

To convert water into gas or *permanent* vapour, requires *elementary* (not external) heat; it requires an igneous matter which will combine with the fluid, and which external heat can never affect. This was accomplished, in the hydrogen, unknown to Watt, who had suggested the *possibility* of converting water or steam into permanent air.*

In my pamphlet I had explicitly stated, that all the experiments proved, *that by passing electric*

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* See Lord Brougham's Life of Watt.

sparks into water hydrogen would be produced—that a metal immersed in water with the addition of heat, (Mr. Grove's experiment) *would also produce hydrogen ;* I therefore concluded that the *water combined indifferently with phlogistic matter of the metal, and with electricity*, and so formed the hydrogen ; and as Dr. Priestley and Mr. Cavendish had ascertained that there is no difference in the hydrogen whether obtained by metal or electricity, (nay, Lavoisier proved that the hydrogen *obtained from plants* in no wise differed from that procured from iron),* I concluded that *electricity*, and the *phlogistic matter which came from the metal, were one and the same matter.*

These were my arguments, which though strong, and unanswerable as they appear to me, seem (judging from the Royal Society) to have gained but few disciples.

I feel however the consolation, that they have not been (not after due deliberation at least) rejected, *as they evidently have either not been read or have not been understood ;* for if they had, however tenacious the Royal Society might be of an opinion which it has, in common with other learned bodies, now erroneously adhered to for more than half a century, it would assuredly not have declared the decomposition of water to be no longer a *vexata*

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* Lavoisier notices the revival of minium *by inflammable air extracted from vegetables by distillation.*—See Watt's Corresp. p. 191.

quæstio, and in a true Catholic spirit, dogmatically pronounce its right to admission amongst the truths of this world—" *semper, ubique, et ab omnibus.*"

Such a course does not carry with it much of the air of philosophy; and I must be excused for saying that I cannot help thinking it highly censurable in any body of scientific men to pronounce absolutely upon a question *so vitally* important to Chemistry as the decomposition of water, without having first acquired a full knowledge of the gases, and for that purpose studied the works of Stahl, Scheele,* Kirwan, Priestley, and Cavendish, and examined their respective experiments relative to *phlogiston;* as in the *absence of that knowledge*, I declare peremptorily that papers on the decomposition of water can only be read as fashionable fairy tales, *and with just as little profit;* and yet it is clear that of these authors, and of the all-important subject of phlogiston, this Committee knew absolutely nothing.

It is evident, from what has been stated, that the two leading experiments, viz. " the decomposition," and " recomposition," of water, admit *each* of them of *two different and totally opposite constructions*, or solutions, while those unfortunately have gained credit, which erroneously favour the notion that water is a compound;* whereas it will be found that the first experiment (decomposition) only proves that *hydrogen is formed of electricity and water;* while the second experiment, (recomposition) is simply *a decomposition of the hydrogen, and a consequent restoration of the water it held in combination.*

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* Our present philosophers were unfortunately wholly unaware what particles of *pure gold* were to be met with in the *mines* of these two neglected Chemists, Stahl and Scheele.

These mistaken views of the two processes may be attributed to two causes; 1st. the *temptation* which so unlooked for a theory as the composition of water held out, and the fascination of the eclat which would accompany it, and which leaped over all difficulties; and 2ndly, the *little that was comparatively known in Mr. Cavendish's time of the electric fluid.*

Notwithstanding, however, all the efforts of the French chemists to involve the subject in inextricable mystery, and the unequivocal manner in which Lavoisier announced the doctrine, a doubt has always been floating in the minds of scientific men as to its validity, and which is evident from Mr. Grove's paper; why else was it considered essential that the Royal Society should tell the world that water can be decomposed by "heat," and that they had rewarded the discoverer of that important circumstance with a golden medal? If water had been already decomposed, of what consequence could it be, whether this could be effected by some other way, or by fifty other ways?

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* See Sir John Herschel's remark in a note to the "Dedication" of this Tract.

But if it had not been satisfactorily proved that water was capable of decomposition, it followed that it might yet turn out, that it was *not* a compound, but an element, as it had always been understood to be from the commencement of the world to the year 1783; and if general utility, if the circumstance of finding this fluid essentially present in all bodies, whether natural or artificial, (with the most trifling exceptions, if any); if, I say, the universal necessity for any known matter, and its general use, are the characteristics of an element, *is there any matter in the whole world more likely to be so than water?*[*]

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[*] With regard to the uses of water, *Lavoisier* observes, that Cavendish and Watt *could find no substance*, not even air, in which water was not present.—Watt's Corresp. p. 168.

Priestley says, that water is the basis of all kinds of gases, and that without it they are not producible.—"Refutation," p. 52.

Watt said *that air was a modification of water.*—See his Corresp. p. 248.

"All aeriform fluids," says Sir H. Davy, "hold in solution a certain quantity of water."—Davy's Elements of Chemistry, vol. i. Again he adds, "*Water is the solvent* employed in chemical processes."—*Ib.* p. 184.

"The whole earth," he concludes, "and its modifications, are acted upon by *moisture* and air; vegetables depend upon it; it occupies two-thirds of the surface of the globe, while it exists also as vapour, air, rain, dew, hail, and snow; so that its effects are constantly connected with the order and economy of our system."—*Ib.* p. 249.

And it is to this matter which the wisdom of our philosophers would fain refuse the rank of an element!

Suppose, then, water to be restored to its station of an element (which it certainly will be), what would then be the present state of chemistry?—with all the analyses—resolving compound bodies into *hydrogen?* &c. &c. *How many thousand volumes* would then be cashiered as useless lumber, and consigned to that other most useful and indispensable element which affords a summary mode of disencumbering shelves of worthless productions.

Was it the contemplation of this scientific perturbation; the alarm at so awful an agitation of the present placid streams of chemical science, that shook the nerves of the Royal Society, and aggravated by the "*Fumam tenebrasque inter nebulasque novembris,*" precipitated the fatal judgment, "that water may be decomposed by heat?"

Notwithstanding, however, this necessarily *infallible* BULL issued from the scientific Vatican of Somerset House, and to which is appended the golden medal, the catastrophe which I have prophesied must *inevitably* happen, and that, too, shortly. The FATES have pronounced the sentence, *and no power of sophistry can stay its execution.**

From *an attentive review* of the early experiments contained in the Authors I have before enumerated,

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* In overthrowing one of the most absurd and mischievous theories that has ever been propounded, the author hopes to be pardoned for any little occasional digression of which he may be considered guilty.

I was perfectly convinced that *electricity had been present in all the productions of hydrogen*, and I recorded that fact *distinctly* and *unequivocally in my former philosophical tract.*

It was then, possibly, to meet this objection of the agency of electricity, that we have Mr. Grove's paper.

But Mr. Grove has either not been aware of, or has overlooked the fact, *that electricity is evolved from metals;* and he has, therefore, contented himself with shewing, that if a *metallic wire* be inserted in water, and sufficiently heated, hydrogen will be generated, and then concludes, that *heat alone* has operated upon the water.

This, however, strange as it is, is not so surprising, as that the Royal Society, and its Committee of Chemistry, should have countenanced that view of the question.

Why, although these accomplished chemists considered Stahl, and Scheele, and Kirwan, and Dr. Priestley, as *fusty*, *antiquated*, and obsolete authorities, who might be safely passed over, had they never read Dr. Wollaston's papers?*

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* Although it is true that Dr. Priestley's "Refutation," published in 1800, did not produce the effect he desired, that I must attribute to the dulness of the readers, rather than to any defect in the author's reasoning. That the "Committee of Chemistry" should, however, never have consulted it (and which is a fact, as it appears to me, indisputable), bears very much the appearance of a preconcerted determination to pass a judgment upon Lavoisier's Theory, "*sans connaissance de cause,*" and at any risk, even that of bringing discredit on the Royal Society.

Now, what does that very able and acute philosopher say, when commenting upon one of his experiments? "We know," says Wollaston, "that when water is placed in the circuit of *conductors of electricity, between the two extremities of a pile,* if the power is sufficient to *oxidate one of the wires of communication, the wires connected with the opposite extremity affords hydrogen.*" "Since *the extrication of hydrogen,* in this instance, *is seen to depend upon electricity, it is probable, that in other instances, electricity may be also requisite for its conversion into gas.* It would appear, therefore, *that in the solution of a metal electricity is evolved during the action of the acid upon it, and that the formation of hydrogen gas, even in that case,* depends on a transition of electricity between the fluid and the metal."

"We see, moreover," he says, in alluding to another process, "that zinc, *without any other metal,* has the power of *decomposing water;* (strange confusion of ideas!) we can, therefore, have no reason to suppose, that *silver* produces new powers, but that it serves *merely as a conductor* of electricity, and thereby forms hydrogen gas.*" (Phil. Trans. for 1801, vol. xci. p. 427.)

As it is quite evident from what has been stated, that the subject of " the decomposition of water" is *by no means understood in all its bearings*, I will endeavour to render it more intelligible by confining the inquiry to the *origin of the phlogistic matter in the formation of hydrogen;* as the clear knowledge and conception of this supposed element of water must necessarily afford the means of arriving at something like a *sound decision* upon the subject.

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* It is remarkable that Wollaston who almost seizes upon the fact that electricity forms part of the metal, relapses into the notion that the latter is *only a conductor* of the fluid, and with an extraordinary chaos of notions talks about zinc decomposing water because it gives out hydrogen.

It may be objected that such an inquiry must be unnecessary, as it cannot be presumed that chemists so skilful and able as those upon whose faith we have assumed that water is a compound, could have come to so important a conclusion without having very accurately examined *both* the alleged constituents of water.

To this objection, (which would come with *little grace* from our present chemists) I answer, that this important matter of hydrogen is composed of *a phlogistic principle, which was not then understood,* viz. electricity.

The galvanic fluid was hardly known, except by the astonishment which certain phenomena produced by it had occasioned. The voltaic battery was constructed *without any knowledge of the principle* upon which it acted ; it was a mere empirical apparatus, the result of experiment upon experiment. Nay, at the very time I am penning these observations, how little is the knowledge which has been acquired about this element, electricity, compared with what might have been gained, if our scientific professors had applied themselves to a theoretic inquiry into the nature of this phlogistic matter; whence it derived its source in the voltaic pile,* and whether it acted merely mechanically, or combined chemically with other bodies, and with what substances; instead of amusing the public with holiday-baubles in electricity; or as the old philosophers would have called such splendid trifles, "*veritates otiosæ.*"

The question whether water be a compound or not, *turns upon the previous question, whether hydrogen is or is not an element;* as, if there be no doubt that the latter is a compound, it is not necessary to demonstrate of what it is compounded, in order to explode the fallacy of the decomposition of water.

Now that it is a compound, is a fact which, even in this stage of the argument, it seems impossible to deny, as every experiment which attempts to *form*

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* It is evident from Mr. Grove's *platinum wire* and Mr. Pouillet's *platinum basin* (see Note, p. 32), that the operation or *rationale* of the voltaic pile is not even now at all understood. I will therefore beg leave to explain it in a very few words. Zinc is used because it is a metal easily *oxidated*, and therefore gives out its electricity freely when acted upon by an acidulated or saline water; but as this electricity would be immediately dissipated, each plate of zinc is accompanied with a *conductor*, or plate of copper, by which means the electric fluid is made to circulate, and thus can be brought to bear upon any particular point or given object. The disks of cloth afford a convenient mode of communicating the acidulated water to the whole surface of the zinc plates, while they thereby aid the transit of the liberated electricity of the copper plates.

water by the combustion and combination of its supposed elements is accompanied *by an explosion and evolution of great heat and light, which, it is admitted on all hands, derive their source from these elements, and which escape from the vessel when the water is forming.*

It is also admitted, for it cannot be disputed, that hydrogen is *highly inflammable,* and as that quality has never been proved to belong to the other element, oxygen, (which, though a supporter of combustion,* does not, by its union with a body, render it combustible), it must follow as an inevitable consequence, that the heat and light which so escapes comes from the hydrogen. *The elementicity of this matter is therefore at once annihilated.* Supposing, however, that the heat and light come from the oxygen, still the doctrine of their being both elements, would be equally overthrown.

Now as all the misconception about water has originated in the ignorance of the phlogistic matter of which this hydrogen is compounded, we will examine this phlogistic matter, and trace its source.

It was questioned in Cavendish's time, whether this phlogistic matter was anything more than the creation of the chemist's brain; a mere "*ens ra-*

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* It is erroneous to call oxygen a supporter of combustion; it merely causes combustion when it is in sufficient quantity to drive away the phlogistic principle from a body, and take the latter's place; combustion being simply the escape of the phlogistic matter.

tionis,"—but I presume, at the present day, our opinions must have completely changed in that respect.

Have we not this phlogistic matter in the electric fluid? Have we it not in an invisible shape in metals and other combustibles? *Is not a metal an earth (sui generis) combined with a phlogistic matter?* a question which is set at rest by Priestley's experiment. Is it not the menstruum which, generated by the voltaic pile, proves a dissolvent of all matters *which heat alone had before been found to operate upon?* Is it not seen in hydrogen, whose inflammation affords the greatest degree of heat yet discovered? Can there then be any doubt of this phlogistic principle being *universal* and penetrating, more or less, every combustible substance in nature?

Priestley and Lavoisier (as we have seen) found it in, and collected it from, plants; and they proved that the hydrogen derived from this source was *identic with the electric fluid, as it would revive a metallic calx,* and reproduce the metal, precisely as the phlogiston obtained from metals, or as an electric current would do. It is allowed too to be the basis of alcohol.*

It would be a waste of time, in fact, to bring forward other examples to shew the existence of such a matter, or its identity with the electric fluid.

Self-evident, however, as the presence of this phlogistic matter appears to be, the doctrine of

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* So says Lavoisier in his Memoir.—See *Watt's Correspondence,* p. 201.

Lavoisier as to the *decomposition of water is grounded upon its non-existence.*

The credulity of, in other respects, clever men, upon this point passes all comprehension. They seem to have been fascinated by a great master in chemical philosophy, and to have lost the power of reasoning. They must have been determined to be charmed, and therefore blinded themselves, voluntarily, to the impressions afforded by common sense. Their maxim was "*in verba magistri jurare,*" let the folly or the consequences be what they might. To speak more to the point, they *unhesitatingly* adopted the assertions of Lavoisier, and then send their senses to wander in the Elysian fields.

Lavoisier treated phlogiston as a phantom, and affected to prove that combustion could take place without the presence of any phlogistic principle. And how did he pretend to shew this? By proving that oxygen united with the burning body. But what then? *whence came the heat?* "Oh! that," said Lavoisier (embarrassed with the question), "that comes from the *oxygen*, which, during its combustion, '*lets go the light and heat with which it was combined while in the gaseous state.*'"* He therefore unequivocally treated oxygen gas as a compound; and, indeed, in all his papers he speaks of the "*principe oxygénique.*" Where were then the *two elementary matters* which formed water?

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* See Lavoisier's Memoir, *Watt's Correspondence*, pp. 175, 200, 207, 213, and again, p. 216.

Incredible as it may appear that any philosopher should be able to impose a system upon the scientific world accompanied with such incongruities, it is nevertheless true, that he positively *denied* the existence of phlogiston,* and then admitting it as the ingredients of one of his constituents, oxygen, (which he said, escaped at the time of its combustion), he concluded by saying, that "*water is the grand reservoir* where nature finds *all the mass of combustibles* that she distributes continually under our eyes; and vegetation appears to be her great medium."

When Lavoisier and Monge distinctly stated that the heat and light escaped from the gaseous matters at the time of forming the water, how could it be possible that the water so formed should be nature's RESERVOIR *for all her mass of combustibles?* and yet this anomaly has proved no obstacle to his theory!

Observe another flagrant fallacy in the arguments of Lavoisier. "Combustion," he says, "is merely the *union of oxygen with a body.*" If so, every body with which oxygen is united would be a combustible; while we know that the metallic earth, when it has absorbed oxygen to saturation, has become *incombustible.*

If it be said that combustion *has taken place* when the metallic base unites with the oxygen, then the oxygen must part with a combustible matter to cause heat and light, or otherwise these phenomena must be the result of the motion of the particles of oxygen entering the metallic earth. If the former, you adopt a phlogistic matter; if motion cause combustion, of what use is the oxygen?

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* "Lavoisier," says M. Arago, "*threw off the trammels of phlogiston.*"—*M. Arago's Eloge of Watt,* "*Watt's Correspondence,*" p. 254.

Oxygen, then, does *not* produce combustion; but it does that *which is evident to everybody who will attend to the process of combustion*, viz.: *it enables the combustible body to part with its combustible matter*;[*] and it is that instantaneous development of the phlogistic matter in quitting the combustible and the quantity liberated, upon which depend all the phenomena we ordinarily mean by combustion.

Can a shadow of a doubt remain in our minds upon this question? Do not we know that an earth and a phlogistic matter form, by their combination, a metal? Do we not know that, by applying an acid to the metal, you can discharge this phlogistic matter, and make it quit the earth, which will then unite with oxygen and form an oxide, as the oxygen takes the place of the phlogiston? Do we not know that this oxigenated earth or metallic calx will *not now burn*—that in consequence of losing its phlogistic matter it is altogether deprived of inflammability; whilst, when in its metallic state, and *before* the oxygen was admitted, it was highly inflammable, and would burn with great brilliancy and light?

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[*] Instead of this function of the oxygen, it has been erroneously called "a supporter of combustion."—See note, p. 57.

What question, then, can there be, that inflammability is the mere act of the inflammable or phlogistic matter, quitting the body with which it was united?

True, it will be said, but we find a metal oxidating without evolving any sensible heat, and yet it must be parting with this phlogistic matter. Most certainly. But, as with ordinary fire, the greater or less degree of heat depends upon the quantity of phlogistic matter evolved at any given time, and our sensation of heat is also governed by the same circumstance, it is clear that the phlogiston may be so slowly given out, and in such small quantities at a time, as not sensibly to affect either us, or other surrounding bodies.

Have we not then irresistible evidence, that in combustible bodies there exists a phlogistic matter? Is it not indisputable that it exists in iron and other metals?

When, therefore, you put a metal into water (which always contains more or less oxygen), and excite the matter by heat, and produce an oxidation whereby the metal parts with its phlogiston, and you find, as the *result* of the process, that you *have an inflammable gas* (hydrogen), to what would common sense say was due the phlogistic or inflammable principle? *to the metal or the water?**

Suppose for a moment that the decomposition of water had never been suggested, and the question I have now put had been asked *for the first time,* would not any chemist, or any man of ordinary intelligence, have laughed at the query—whence comes the inflammable matter?

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* See Note, p. 56.

And yet the proposition of Lavoisier's is precisely this—he insists upon your abandoning the metal, where the inflammable principle is *proved to exist,* and that you attribute the inflammable principle *to the water, the most powerful of anti-combustibles,* and the one universally resorted to to extinguish combustion ever since the creation of the world.

Really, when this subject is divested of its complexity, and is placed clearly and distinctly in its proper light, it is hardly possible to believe that (I really blush to use the word "philosophers") any class of mankind could be the victims of so great an imposture, as that water can be decomposed, and *believe, too, that it is the "reservoir of combustibles!"*

I know that it will be asked by those, misnamed philosophers, who cling to error and delusion, from the dread of unlearning what has been the cherished doctrine of their lives, "How comes it that oxygen is found to accompany this phlogistic matter of hydrogen?" Now the answer to this question is one of the easiest things possible; but as I am most anxious not to divert the attention of the reader from the other and most important constituent, hydrogen, I shall consign the answer to this unimportant question to a note.* It, will, however, allow me to say, be time enough to talk of oxygen when

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* The difficulty about the appearance of the oxygen, is a subject of very minor importance.

The water which is operated upon contains it in abundance, although chemists are not agreed as to the quantity it will absorb. One thing is, however, clear, viz. *that there is no means of excluding it from water.* This fact is established by Davy in his experiments given in the Phil. Trans. for 1807, page 1 to 56, in which gold and agate cups were used, and the water previously subjected to every means of purification.

Dr. Priestley says, that he *boiled* water, and examined it when warm, and found that it contained nearly as much oxygen as before.—See the Appendix to his "Refutation."

But the great absorption of oxygen by water is proved by Scheele's experiments with leeches. He put some leeches in a glass bottle half filled with water, which he closed, and found them dead in the course of two or three days. On examining the air above the water, he found that it extinguished the flame of a candle. He afterwards put some more leeches in the same bottle, half filled with water, which he covered with a piece of crape, *when they lived for two years without changing the water.*—See "Scheele on Air and Fire," p. 166.

This experiment shews that these animals cannot exist without oxygen; and it also shews that a small quantity of water hermetically closed, contained *enough oxygen* to support the leeches two or three days, and that water when exposed to the atmosphere is always charged with oxygen.

From oxygen being indispensable to the existence of animal life, and therefore required in the lowest depths of the ocean (certainly not less than six miles, but estimated by Laplace at nine,) its facility of combination with water must be very great, and the supply inexhaustible.

The Professsor of Chemistry at Geneva, M. de la Rive, has *we have thoroughly investigated and understood the other constituent, hydrogen;* and when we have done with that, I suspect, indeed, I am quite sure, that our *anxiety about oxygen* will have very considerably abated, if not wholly vanished.

I have already mentioned, what I think it right to repeat in this place, that notwithstanding it has been supposed that Lavoisier discarded "phlogiston," *he did no such thing:* he only *shifted it,* as we have seen, from one substance to another, (from the hydrogen to the oxygen) and at length said it existed in the *water*, which was, he insisted, "*the reservoir of combustibles.*" He therefore discarded the *name* only of phlogiston, and left the substance as before. Though the existence of a combustible matter is explicitly stated by Lavoisier several times in his different papers to the French Academy; (for how could it be disputed?) yet the contrary notion seems to have prevailed, merely because he repudiated the word "phlogiston." In fact, there seems to have been, and even is now, such a fatality attending this question, that any clear and distinct ideas about it must be looked for in vain.

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shewn, that in the process of decomposing water by the voltaic battery, oxygen is carried to the water by oxidated particles of metal contained in the electric current.—See "Archives d'Electricité," par M. de la Rive.

It is to be remembered that the oxygen found in the process of the supposed decomposition of water is said to be *only one half in volume* of that of the hydrogen.

Dr. Thomas Thomson (whose excellent work upon Chemistry cannot be too highly appreciated, and who evinces in his observations a very superior intelligence), says, to my great astonishment, "Thus Lavoisier explained combustion," (alluding to the theory I have already explained) "*without having recourse to phlogiston,* a principle *merely supposed* to exist because combustion *could not* be explained without it." "*No chemist*" (what follows gives us the unfortunate cause of our present delusion) "*had been able to exhibit phlogiston in a separate state, or to give any proof of its existence, except only its convenience in explaining combustion. The proof of its existence consisted entirely* in the IMPOSSIBILITY of explaining combustion without it."

Now we have seen that Lavoisier's explanation of combustion does not get rid of phlogiston, but, on the contrary, that he gives his phlogistic matter first to oxygen, to which he attributes the combustion, and which he tells us distinctly it parts with on being burnt with hydrogen; and then says that water is the source of combustibility.

Dr. Thomson, however, goes on to say, "M. Lavoisier, therefore, by giving *a satisfactory* explanation of combustion *without having recourse to phlogiston, proved that there was no reason for supposing any such principle at all to exist.*" Now does not this demonstrate an extraordinary delusion in such a man as Dr. Thomson? Allowing for a moment that a notion so outrageous could enter any man's mind as that a body can burn in the absence of phlogistic matter, and *merely by the absorption of a non-combustible,* as oxygen is, how did Lavoisier prove this *when he erroneously gave the phlogistic matter to the oxygen?* Dr. Thomson, however, (mark well) when he penned this ill-considered comment, *felt conscious of its unsoundness,* and in the following sentence, he exclaims, "*This did not overturn the hypothesis of Mr. Kirwan, who made phlogiston and hydrogen synonymous, because there could be no doubt that such a substance as hydrogen actually exists—but hydrogen, if it be phlogiston, must be a component part of every combustible.*"* To be sure it must, if it were so; but as hydrogen only contains the phlogiston, hydrogen is not a component part of every combustible, but its electric or phlogistic base is.

We have here, in this spontaneous development of Dr. Thomson's mind, and its momentary liberation from the trammels of an absurd theory, which would not for a moment have been tolerated if any man less eminent than Lavoisier had been its author; we have, I say, in these few lines the real theory of combustion. It is the phlogistic matter united to a substance, which constitutes its combustibility—but, that matter is *not hydrogen;* and here is the "*fons et origo mali;*" *but the phlogistic matter, which is the igneous base of the hydrogen,* the latter being only a *combustible body*, compounded of phlogiston and water.

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* Thomson's Chemistry, vol. i. p. 140.

The unfortunately mistaken identity of hydrogen with phlogiston is the rock upon which all the chemical philosophers have been shipwrecked. They could never *separate* the combustible matter from this compound substance, hydrogen gas.

Everybody, as Dr. Thomson admits, felt the necessity of a phlogistic matter, as it made the doctrine of combustion intelligible. Phlogiston was of great convenience, he says, and therefore insisted upon; but it could never be found in a concrete or distinct state; and hydrogen gas would not answer this description, as in all the processes in which this matter was used to revive metals, though the phlogistic principle disappeared, *and had evidently united itself to the metallic earth*, there was always *something remaining behind;* some residual matter, besides hydrogen (water for instance), *and which could not have happened if hydrogen had been pure phlogiston.*

But without phlogistic matter how can combustion be rationally accounted for? What is heat, if it be not a substance? Is it the mere effect of motion? So thought Bacon* when the electric fluid was only known as existing in the clouds, and hardly that. Is heat the result of a rapid separation of the particles of matter? But this conclusion could not satisfy the mind, when it considered the *intensity* of heat from furnaces, and other processes of combustion. The necessity of *some substantial principle of heat* always pressed upon the imagination as indispensable to the explanation of a multitude of phenomena connected with ignition. But if it exists, said the inconsistent and importunate Pyrrhonists, show it us in a distinct and palpable shape. This palpable shape, however, escaped the intelligence of the Phlogistians, and this "*hiatus maxime deflendus*" was taken advantage of by Lavoisier to palm upon the world a theory which, whatever were the motives which produced it, has proved the subversion of chemistry.

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* See his treatise "*de formâ calidi.*" N.B. Combustion is not caused by the motion of every kind of matter, but by the motion of a *peculiar* or *combustible* matter.

The different stages of the phlogistic doctrine were these. Becher insisted that *sulphur contained an inflammable matter*, and was a compound body. Stahl continued to maintain the doctrine of the existence of an inflammable principle, and brought forward experiments which gave it vast probability; but Mr. Cavendish made the greatest advance by afterwards finding out hydrogen (then called "inflammable air.") Dr. Priestley *thought this hydrogen was the true phlogiston*, and that his experiments had established that fact; and Mr. Kirwan distinctly insisted upon their identity.

Dr. Priestley was, however, shewn his error by Mr. Cavendish, and Mr. Kirwan being thus overthrown, and unable to meet the unceasing cry of the French chemists to produce this phlogistic matter, abandoned his opinion, and became a convert to the water-decomposers. But Dr. Priestley saw too plainly the existence of phlogiston, and opposed the water theory to the end of his life.

Lavoisier contended that the phlogistic principle was purely imaginary, and that instead of the metal yielding any such matter, it merely decomposed the water by *the affinity* of one of the constituents of this liquid (oxygen) for the metal; and that the *inflammable air was the other constituent of the water.*

Thus has this *most unsatisfactory and improbable doctrine* continued to be the order of the day, (notwithstanding the *decided opposition to it* on the part of Dr. Priestley, who published, as we have seen, as late as the year 1800, a work expressly to refute it), and WATER has been allowed (under the sanction of Lavoisier) to be the "RESERVOIR OF COMBUSTIBLES," only waiting for the splendid and crowning experiment of Mr. Grove, and the solemn approbation of the ROYAL SOCIETY, to be irrevocably emblazoned on the Tables of *eternal and immutable* TTUTHS.

This achievement of Lavoisier's will be a standing memorial, an indisputable monument, of the power of a man of genius to impose upon mankind without being himself deceived; for I think it impossible to come to the conclusion that so clear and able a philosophical chemist, and one so intimately versed in all the experiments in which phlogiston was so *manifestly* apparent as Lavoisier must have been, could have been himself the victim of the imposture.

Why, besides water and a metal producing hydrogen, charcoal did the same thing; and before Lavoisier broached his new theory, he *admitted* of a combustible matter in charcoal, and called it "*le principe charbonique.*" Mercury, too, distilled in *oil of vitriol* to dryness, gives out all its *phlogiston* to the oil, with which, says Dr. Priestley, *it is loaded.* Alcohol is also largely charged with it. Quartz and flint pebbles heated in water yield it. *Zinc made red hot, and steam passed over it*, gives out hydrogen copiously; BUT NO OXYGEN IS FOUND WITH IT, NOR DOES ANY EXIST, AS THE CALX HAS NOT INCREASED IN WEIGHT AFTER THE LOSS OF ITS PHLOGISTIC PRINCIPLE. Dr. Priestley tried this calx, he says, in every possible way without detecting a particle of oxygen.*

Electricity and heat are identified in their effects, as eggs can be hatched as well by the one as the other.†

"Both electricity and heat," says Dr. Thomson, "will *fuse* metals—both will *kindle* combustible bodies—both will dilate bodies—"*electricity,*" *he says,* "*has great influence in the heating and cooling of bodies.*"

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* If there be no inaccuracy in this experiment, it would seem that there are some bases which do not imbibe the oxygen when it eliminates the phlogistic matter. The reverse is however the general rule.

† Mr. Archard's Exp. Nicholson's Journal, xxvi. p. 30. Thomson's Chemistry, vol. i. 159, 160.

"I do not mean," says Dr. Thomson (alarmed lest he should be thought to forsake the fashionable creed of the decomposition of water), "to draw any other conclusion from these experiments, *than that electricity is very often concerned in the heating of bodies, and that probably some such agent is employed in accumulating the heat produced by friction.* May not then," he asks, "this *be the source of the caloric which appears during friction?*"

Dr. Thomson remarks, as an extraordinary circumstance, that when some gaseous bodies unite to form a solid (as ammoniacal and muriatic acid gases) *a very considerable quantity of heat is evolved at the moment of their union.*

From electricity again setting fire to gunpowder, to alcohol, and other combustible bodies, it is obvious, says Dr. Thomson, "*that electricity evolves both heat and light.*"*

He also observes that "*when metals are ignited by electricity they remain longer red hot than when acted upon by common fire.*"

I shall now proceed to make some further remarks upon phlogiston, *to which I entreat particular attention.*

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* Same work, pp. 163 and 164. "*The smoke of all volcanoes are pregnant with electric fire,*" says Sir W. Hamilton in his "Campi phlegræi," speaking of Vesuvius.

It has been seen that Dr. Thomson says (alluding to Mr. Kirwan's notion that *hydrogen* was the *phlogistic principle)* "but if hydrogen be phlogiston, it must constitute a component part of every combustible, and it must separate from the combustible in every case of combustion."

Now Dr. Thomson knew when he said this, that hydrogen did *not* form a part of a *solid* combustible body, and *therefore that it could not be the phlogiston itself;* and there are two experiments which decide this point, and which I request my readers to ponder well in their minds.

Preparatory however to citing these experiments, I will beg to call to the reader's recollection that a *metallic calx* or oxide may be revived or restored to its metallic form and character, either by passing through the calx a stream of *electricity* (Beccaria's experiment), or by heating the calx in *hydrogen gas* (Dr. Priestley's experiment and often tried); *and that the necessary and inevitable conclusion to be deduced from* these experiments is, *that one and the same phlogistic principle is common to electricity and hydrogen.*"*

I shall now proceed to give a summary of the two experiments, and to which I have already requested very particular attention.†

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* Mr. Cruikshank discovered *that the same agency which evolves hydrogen from water will revive metals from their solution in acids.*—Singer's Elem. of Electr. p. 370.

† These two most important experiments of Dr. Priestley's, are to be found in his works upon Air, &c. vol. 6. pp. 73-85, and

Dr. Priestley says that by the help of a burning glass, he heated a bit of iron, enclosed with *pure oxygen* in a glass vessel, when he presently perceived the oxygen to be diminished and *visibly absorbed by the iron,* which was converted into a slag (oxide), and had gained a weight nearly corresponding with that of the air absorbed.

He afterwards (he says) heated in the same manner this slag (oxide) *in inflammable air*, enclosed in a glass vessel, when he observed the *inflammable air to disappear*, and that a considerable quantity of water was produced. *That the greater part of the slag had recovered its metallic state*,* and the matter had *lost in weight nearly equal to that of the water given out.*

Now I say it is self-evident *that this water resulted from the decomposition of the hydrogen, and was merely the fluid which this matter had held in solu-*

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are frequently noticed by Watt, and particularly in his Letter to Dr. Black, of the 21st April, 1783, and in another letter, to Mr. Hamilton, of the 22nd April, 1783, which are given in Watt's Correspondence, pp. 13-24, and it is from these experiments that Watt incorrectly concluded (as Kirwan had done before him) "*that pure inflammable air is phlogiston itself.*" See Preface to his Corresp. xlvii.

I here unequivocally reiterate that when these experiments are *perfectly and fully comprehended* (*and which has never yet been the case*), the whole mystery about the decomposition of water is unveiled, and the delusion of the theory at once seen through.

* The oxide of iron, Lavoisier says, is *never wholly revived.* Most probably, for want of a sufficient quantity of hydrogen or phlogiston.—See his Memoir, Watt's Corresp. p. 191.

tion; and that this water had absorbed some of the oxygen which had quitted the slag (oxide), and was thereby rendered heavier than *the hydrogen gas* employed in the process.

This must be the fact, for the following reasons: First, that all the phlogistic matter of the hydrogen must have been (as we have seen) imbibed by the calx in order to revive it. Secondly, that there therefore could not have been hydrogen enough to form the water which requires *two volumes* of hydrogen *to one of oxygen,* according to the theory. Thirdly, that the weight of the water equalling that of the oxygen gas contained in the iron, precludes the possibility of hydrogen gas having assisted in forming the water, as in that case there would have been no hydrogen to restore the metal. Iron, as we have seen, requiring a considerable quantity of phlogiston for its revivification, and being never found, from that circumstance, wholly revived.

If, however, the reasoning from these experiments should not be considered conclusive, I will give another that will leave no doubt that hydrogen is *not an elementary phlogistic matter*, but a compound *containing phlogiston.*

The experiment in question is the combustion of the diamond, first spoken of, I think, by Dr. Priestley, though now exceedingly common.

We have already seen the various matters in which the phlogistic principle is discoverable. For example, in hydrogen, produced from a metal, water, and heat; from charcoal, ignited in water; again, from mercury, when distilled in oil of vitriol, when there is evolved a great quantity of phlogiston; again, from zinc made red hot, and steam passed over it, when it gives out hydrogen freely, and without imbibing oxygen, according to Priestley. From iron, by the same process, which does the same. But all these processes, say the water-philosophers.* prove only the decomposition of water.

Now the diamond experiment, I answer, will shew the phlogistic matter in *a distinct shape from that of hydrogen.*

The composition of the diamond is undisputed. It is admitted to be pure carbon, united to a phlogistic matter. Sir I. Newton, with that penetration which accompanies a clear head, predicated of the *diamond* that it was the *most combustible substance in nature*, little imagining that it would ever be asserted by any philosopher with a "*mens sana*" *that this combustible matter must be sought for in a bucket of water.*†

When the diamond, then, is enclosed in a glass vessel containing oxygen gas, and ignited, the result is a brilliancy of flame and light, until the diamond is wholly consumed, when the only matter found in the vessel *is carbonic acid gas.*

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* I can't call them *Anti-phlogistians,* as they do not, as I see, deny a phlogistic principle, but merely quarrel with *the name.*

† The Philosopher of Malmesbury has said, "to the privilege of absurdity no creature is subject but man; and of man those are of all most subject to it that *profess* PHILOSOPHY."

The mind of Sir Isaac Newton was of too high an order to wish, like Lavoisier, to repudiate an element, because it was beyond the grasp of his comprehension, and could not be made a docile instrument in the hands of every disciple of Science.

We have, however, here no water. Why? Clearly because there is no hydrogen. You have, however, a phlogistic matter which gave the heat, and light, and combustibility to the diamond, and *which has escaped;* and yet that phlogistic matter is plainly not hydrogen, because if it had been, it must, by Lavoisier's theory, have *united with the oxygen gas* and have formed water.

We have then *distinctly and unequivocally in the diamond a phlogistic principle, which is not one of the elements of water.*

The only materials found after the combustion of the diamond, are the carbon and the oxygen combined in a gaseous state, and forming what is called carbonic acid gas.

It is evident, however, that these materials were not those of which the diamond was constituted, as the matter left is INCOMBUSTIBLE. We know in fact that the oxygen gas was added to the diamond to aid the combustion of it, and therefore that all that is left of the diamond is the carbon.*

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* The power of chemical analysis I am, however, satisfied is very much exaggerated. *How many substances escape the observation of the analyser?* Professor Liebig, with all his tests and tact, is unable to distinguish the mephitic atmosphere which generates diseases in their worst form, from the air of the Swiss mountains. The tainted atmosphere of a crowded theatre, and

The carbon, however, is clearly not the diamond; but only one of its constituents. What, then, was the other constituent? *Why that matter which rendered it the most inflammable substance in nature. The matter which made it combustible.*

Suppose our Lavoisiers to say that the inflammable principle which the diamond has lost by combustion was hydrogen; and suppose them to add to this, that the reason the hydrogen and oxygen did not form water is, that the oxygen has a *greater* affinity for the carbon than for the hydrogen. Be it so; but then I ask, *what has become of the hydrogen?* Where is it? It is not in the vessel; and we know that from an hermetically closed vessel hydrogen will not escape, though pure phlogiston will do so, as the delusive experiment for forming water by the gases proves!

But I would here caution the water-decomposers against multiplying the number of bodies whose attractive power for oxygen is such as to decompose the water, as it may soon amount to a miracle that we have any water at all—that any is left us *undecomposed.*

I will just remark, by way of an *obiter dictum*, to those who may not have formed a *just estimate of this extraordinary affinity* which the Lavoisiers make ca-

that we breathe on a spring morning, are both alike to the investigation of the chemist. *How far is this from detecting the effluvia which indicate to the hound the track of the deer, or to the spaniel the proximity of the partridge?*

pable of separating hydrogen from oxygen in the decomposing-water, what we are told by Mr. James Watt, viz., that, water may be expanded *by heat* until brought into a state of steam *occupying* 18,000 *times the original bulk of the water*,* and that without the least approach to decomposition. *This may afford a glimpse of what must be the power of such an affinity, and consequently of the probability of its existence.*

Is it not indisputable, then, that when this phlogistic principle (one of the constituents of the diamond) unites with water, it forms hydrogen, and that when it unites with carbon it forms another substance?

We here then see distinctly the phlogistic principle under *two* different and opposite combinations:—first, as hydrogen, and secondly, in another matter, which is not hydrogen, but a diamond. If it had been hydrogen it would have united with the oxygen, and formed water, according to the theory; but as I say, and insist, it would have been decomposed, and *given up its water.*

If it be pretended that the oxygen united to the carbon *in preference* to the hydrogen, then I say—*shew me the hydrogen* (which *cannot* escape from the vessel like phlogiston), but which is not to be found.

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* See Dr. Thomson's Elem. of Chem. vol. i. p. 180; and yet Mr. Grove and the "Committee of Chemistry" suppose they have succeeded in this decomposition by "*the heat*" *of a wire of platinum.*

The same thing is proved by the experiment I have given of Dr. Priestley's, in distilling mercury to dryness in oil of vitriol, when part of the acid comes over, he says, "*loaded with phlogiston.*" Now in this process *you have the oxygen and the phlogiston, but no water.**

Dr. Priestley proved that when *charcoal powder is perfectly dried, and from which the air had been most carefully expelled*, and a metallic calx (oxide of iron) is placed in it, that the metal will be revived on the application of heat.† Lavoisier states also the same experiment and result.

Here again you have the oxygen in the calx or oxide, and the phlogiston, but no water; as you have here no hydrogen.

Now we have seen, and know, that when charcoal is heated *in water*, hydrogen is copiously evolved: so that you find in this process *of the restoration of the metallic calx by charcoal powder, the same phlogistic principle you have in hydrogen.*‡

Scheele says, that he put a portion of the earth of gold, mixed with some *dust of charcoal*, into a small glass retort, and set it on *hot sand*, *when the gold earth was reduced* (revived), *and the coals were kindled.* Now, says Scheele, *the heat of the sand* could *not* have been the cause of the "*recension*" *of the coals,* and which he found out *by strewing part of the dust of the charcoal on the sand, which was not kindled.*[*]

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* Priestley on Air, &c. vol. vi. p. 109.

† Mentioned by Cavendish. See Watt's Corresp. pp. 103 and 146.

‡ See Watt's Corresp. p. 103. Also, Mr. Kirwan on Phlogiston, p. 105.

Dr. Priestley, who employed himself a good deal in the revivification of metallic calces by the aid of hydrogen, has given a table of the quantities of hydrogen which different calces absorb for that purpose, as he found that the metals varied with regard to the amount of phlogiston with which they combined.[†]

He also ascertained that nitrous air and inflammable air (hydrogen) contained nearly the same amount of phlogiston ; the first being evidently the union of phlogiston with nitrogen or azote, and the last with water.

The existence of a phlogiston distinct from hydrogen is proved by Scheele, from the effervescence which takes place when the noble metals (gold and silver) are acted upon by nitric acid,[‡] by the *red colour of the vapour* which they throw out ; and this is established from the circumstance that the *calces* or oxides of these metals (that is when the phlogiston has left them) when dissolved in the same menstruum *give out no such vapour.*[*]

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[*] Scheele on Air and Fire, p. 146.

[†] Priestley's "Refutation of the Theory of Water," pp. 34 to 40.

[‡] I have not thought it necessary to change the chemical terms used before Lavoisier's nomenclature, and I give them as they are found in the authors I cite.

It is, therefore, evident that the red vapour is caused by the phlogistic matter in the metal,

Dr. Priestley also shewed that common air enclosed in a glass vessel *diminished considerably in bulk* upon having *electric sparks* passed through it, and that upon introducing a test paper of litmus it acquired a red colour; and upon further examining the result, he found that *the azote was united to a phlogistic matter.*

Mr. Cavendish, in pursuing the same experiment, found that upon mixing the oxygen and the nitrogen in different proportions to those which form our atmosphere, and upon passing the electric sparks through them, both gases disappeared and were converted into *nitrous acid.*

Now here again is irrefutable evidence of a phlogistic matter given by electricity to these gases, by which a new phlogistic substance is produced.

But, observe that the agent to produce this is the *electric fluid, and that no other matter will have the same effect.*[†]

Surely, after these satisfactory and convincing experiments, and which are to be found in the works of Stahl, Scheele, Kirwan, Cavendish, Priestley,

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[*] Scheele on "Air and Fire," p. 78.

[†] See Dr. Priestley's Refutation, &c. pp. 44 to 46. See also Dr. Thomson's "Elements of Chemistry," vol. i. p. 209.

Lavoisier, Monge, and Berthollet, (and are also mentioned in the first volume of Dr. Thomas Thomson's fifth edition of the " Elements of Chemistry," and again in the second edition of his separate work on " Heat and Electricity,") there can be no hesitation in admitting the existence of a phlogistic principle or matter in other *forms*, *i. e.* combined with other substances besides that in which we find it, in hydrogen gas ; and if this conclusion *be irresistible*, (and how can it be questioned?) I should apprehend that the faith of the " Committee of Chemistry," in the decomposition of water, must begin to be a little *decomposed;* or, to use another form of the same root, a little *discomposed.*

But I should, indeed, exceedingly regret if the repudiation of this marvellously silly theory rested upon these grounds alone, (unanswerable, as I think, they unquestionably are), as there might possibly be some water disciples, so pertinaciously attached and wedded to it, as still to foster some " longing, lingering" hope of its veracity.*

This, however, must not be, for *I feel that I possess the power* of wholly dismantling this *"cloud-capt"* THEORY ; of dissolving this " *baseless fabric of a vision," without leaving one solitary* " *wreck behind."*

Let us observe, then, and mark well, the reason which Lavoisier gives for the decomposition of water by means of a metal (zinc or iron), viz. : the *vast*

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* ———— *" once wedded fast*
To some dear falsehood, hug it to the last."

affinity which the oxygen of the water has for the metal; an affinity so extraordinary, that it surpasses that which oxygen has for hydrogen, and which *heat, that separates all other things, cannot* (*malgré Mr. Grove's experiment*) *effect;* for though "water," says Dr. Thomson, "converted into steam expands more than any other liquid hitherto examined*—*it is never separated by heat, when every other matter is,—except alcohol;*" and we have seen also that Dr. Priestley exclaims, "why cannot heat alone, *and without the aid of a metal,* convert water into air?"

The *intensity* of adhesion, then, between the oxygen and the hydrogen, when it resists so powerful a dissolvent as heat, *must be indeed prodigious;* but the decomposition of water is based upon the fact of this affinity being *surpassed by the still greater affinity which oxygen has for a metal, iron for instance.* It is, says Lavoisier, in consequence of this greater affinity which oxygen has for iron over that which it has for hydrogen, which separates the oxygen, and decomposes the water. Be it so!

Now we know that a metallic calx (oxide of iron, for instance) may be restored to its metallic state by being *heated in hydrogen;* and in which operation the calx parts with its oxygen, and absorbs the hydrogen, (or, as I insist, the *phlogistic principle in the hydrogen*), in its stead.

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* Until it is 18,000 times the bulk of water. See *Thomson's Chemistry*, 5th edition, vol. i. p. 180. See also p. 63.

Here, then, we find that this most astounding affinity which oxygen is said to have for iron earth, and which causes it to quit its other element, and thereby decompose the water, is in this operation *miraculously changed*, and that the *hydrogen now possesses a greater partiality for the iron earth, or calx than the oxygen has for it*, and that, in consequence, the latter element politely makes its bow, and leaves the hydrogen to take its place, and which the latter does, *sans cérémonie*, and forthwith *revives the metal* with its wonted brilliancy.

Now this exchange which the metallic earth makes of hydrogen for oxygen, or oxygen for hydrogen, reciprocally, establishes the fact, and the only fact, viz., that of its *indifferently* receiving either substance.

It is not the force of the external heat which can be said to be the cause of the metallic earth's either receiving or rejecting the hydrogen, as the heat used to reconstruct the metal by means of the hydrogen, is much less than that used when the gun barrel in Lavoisier's experiment is heated to receive, or combine with, the oxygen.

The pretext, therefore, that it is the superior affinity which oxygen has for iron which causes the decomposition of water, falls at once to the ground.

Now this little dilemma never once occurred to any of the deep-thinking philosophers who have allowed themselves to be the dupes of the French chemists.

Have I not now scattered this quixotical theory to the winds? Is not the whole army of water-decomposers overthrown? If this be denied, I must exclaim, with Locke, that "Victory no more always accompanies TRUTH than it does right!"[*]

Must I, then, resume the battle? Be it so! and it shall be renewed with such a reserve of artillery as shall sweep the field!

We remember, that when water is said to be formed of the two gases (hydrogen and oxygen) by means of an electric spark which ignites them, a loud explosion follows, when an intense heat and light escape through the pores of the vessel, and which when cooled is found to contain water, only. Now this heat and light, which so escape, are noticed particularly by Monge and Lavoisier, who say, in their Memoir to the French Academy, "that the water cannot be said to come from the two airs *alone*, because of the *inflammable matter and light* which escaped and could not be retained."[†]

This heat, says Dr. Priestley, is so great that he could not hold the vessel, and that as the DEW *formed by each operation is very trifling*, and it is necessary to feed the apparatus from time to time with fresh gases, he was obliged to cool the vessel after each explosion. This heat, Dr. Thomson says, is the greatest yet discovered.* It is clear, then, as the French chemists say, *that light and an inflammable matter escape* on the combination of the gases, and consequently the water, (as Lavoisier and Monge admit), resulting from the operation, is the combination of the two gases, *minus the inflammable matter and the light.*†

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[*] Life of Locke by Lord King, vol. ii. p. 153.

[†] See this Memoir in Watt's Correspondence, p. 216.

Now then comes a dilemma, in a shape more formidable than the one I have already brought forward, and which will prove not less appalling to the water philosophers, than did the ghost of Banquo to Macbeth, nor less fatal than the spectre which frowned upon Brutus on the eve of the battle of Philippi.

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* Who can now doubt that earthquakes and other minor convulsions of the earth, are the effects of the same cause as that which our philosophers imagined had formed water, viz.: the deflagration of hydrogen in oxygen gas? And who can doubt that this hydrogen is due to the decomposition of the metals and other combustibles in the bowels of the earth through the medium of water? Dr. Thomson says, "Messrs. Fox and Henwood *leave no doubt of the existence of electric currents in mineral veins.*"—See his work on "Light and Heat," p. 540.

† It was this escape of the phlogistic matter that encouraged Lavoisier to declare that he had *got rid of phlogiston*, and which Lord Brougham eulogises as a great advantage to chemistry. "The overthrow of the *phlogistic theory* (says his Lordship) and the happy generalization upon the combination of bodies make him regarded as *the first reformer of chemical science*, and the principal founder of that MAGNIFICENT FABRIC which now fills so ample a space in the eye of every student of nature."—Life of Lavoisier. Now, how very strange that so sagacious a critic should not have seen that this so happily banished phlogistic matter is a component part of one or other of the two gases, of which water is said to be made, and must be sought for again before those gases can be re-constructed.

This very identical water, thus formed by the two gases, is also capable of being decomposed—of being brought back to its elementary state by a heated piece of platina wire, when each of the gases will re-appear; and this transformation from water to air, and air to water, may be effected, *ex necessitate rei, ad infinitum; i. e.* as long as a drop of water remains.

Now, as the inflammable matter escaped from the gases when the water was formed, will the water-philosophers tell me from what source the inflammable principle—the heat and light are derived, when the same water is again and again decomposed, and oxygen and hydrogen are again and again reproduced, and that these processes continue in succession, up to the last remaining particle of water?

Will these experienced chemists say that these gases (both inflammable according to Lavoisier, but one, hydrogen, notoriously so,) can be reconstructed out of the water, *without the aid* of the phlogistic matter which escaped when the water was formed?

Can there be a doubt, now, that this inflammable matter does *not* come from the water, but is derived *aliundè, i. e.* from some foreign matter? and what can this matter be but the metal, where the phlogistic matter is proved to exist, and which is made use of to decompose the water.

Is it not passing strange that this self-evident and fatal dilemma should never have presented itself to any of our modern philosophers? not even to the "Committee of Chemistry of the Royal Society," gentlemen *so conversant with the theory of the "Decomposition of Water," and so intimately acquainted with all the experiments connected with the subject*, as to be able to pronounce dogmatically and ex cathedrâ, that "*heat alone*" is sufficient for the purpose? Do these philosophers still entertain the belief, that "heat" will decompose water?

It is unquestionably superfluous to offer any further objection to this already fallen theory, but in compliment to Dr. Thomas Thomson, I will notice a difficulty which occurred to him about the *condensation* of the gases. After giving the weight of the hydrogen and oxygen, the Doctor says, (in speaking of the formation of water) "it follows *that* 1700 *cubic inches of the constituents of water*, when they combine together, and assume the form of *water*, occupy little more *than one cubic inch. So that the particles approach nearly seventy-two times nearer each other than when in a gaseous state.*" "This," says Thomson, "*is to be sure an enormous condensation!*"*

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* Dr. Thomas Thomson on "Heat and Electricity," 2nd Edit. p. 254.

To this *improbable condensation*, I would ask for the probability of such masses of gaseous matter being employed by the OMNIPOTENT to form *the unfathomable ocean*, connected with the fact of our atmosphere only containing one of the gases in a comparatively small quantity, and of the other nothing, except by accident.*

I will now proceed to state the objections to the theory, *which the water-philosophers have themselves discovered*, with the answers they have given to them, and which will be found *no less profound than amusing.*

Sir Humphry Davy made some very accurate experiments† in order to ascertain the effect of connecting one extremity of a voltaic battery with *distilled water*, contained, first in an agate cup, (and afterwards in a gold one) protected from the air; and the other extremity of the battery connected with distilled water contained in another similar cup; the respective cups of water being *placed at such a distance from each other*, (several feet) *as to prevent the current of electricity from making a continuous circuit;* in other words, *so that there might be a chasm or interruption in such circuit.*

It was, nevertheless, found, says Davy, that *the work of decomposition had gone on in each cup.*

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* Mr. Cavendish says, that 500,000 grain measures of hydrogen and 1,250,000 like measures of common air, *produce* 135 *grains of water*. Phil. Trans. Abridg. vol. 15. p. 437.

† Phil. Trans. for 1807, pp. 1 to 56.

That the water in the cup connected with the zinc extremity of the battery, or positive end, had given out some bubbles of *oxygen only*, while the other cup, at the negative extremity of the battery, had given out *hydrogen gas exclusively*.

Now all these bubbles had their origin (as is insisted) in, and were caused by, the *decomposition of the water*.

But as the decomposition took place in *each* cup, how did it arise that the *two gases or constituents were not found in each cup*, instead of hydrogen in the one, and oxygen in the other?

Now, if Lavoisier's theory be a sound one, it is not to be overturned by such trifling objections, and so thought its supporters, and the requisite and *convincing* answer to this difficulty was therefore at hand; but it certainly required the mind of no ordinary philosopher to deal with it, as we shall see from the following passages:—

"The most difficult feature" (says Dr. Henry) "of all the voltaic decompositions, is the *invisible form* in which the *separated elements of various compounds appear to traverse the fluid, and arrange themselves at the opposite wires.* The oxygen and hydrogen that appear in some of our experiments *at the distance of three feet from each other*, are *necessarily* supposed to result *from the same particle of water*, and if this be situated at either wire, one of its elements (either the oxygen or the hydrogen) *must pass through the whole length of the tube to* *reach the other*, and that in an *invisible state*, for the gases are separated at the opposite extremities *without any apparent alteration of the interposed fluid*."

"On the hypothesis of electric energy, the hydrogen is said to be attracted by the negative wire, because it is naturally positive; and the oxygen to the positive wire, because it is naturally negative; this does not, however, explain how the same particle of water can have its elements liberated at so great a distance from each other; and to account for this fact, according to that hypothesis, it is necessary to suppose that the particles of water between the wires are arranged with their elements in juxtaposition, *like two parallel rows of beads*, the one hydrogen, the other oxygen; as the decomposition goes on *these are supposed to slide past each other*, so that each particle of oxygen comes successively in contact with different particles of hydrogen. In other words, an atom of hydrogen escapes at the negative wire, and at the same moment an atom of oxygen is attracted to the positive wire; the number of atoms of oxygen and hydrogen between the wires are therefore still commensurate to each other, *and have only changed places*."*

In speaking of this hypothesis of Dr. Henry's,

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* Dr. Henry on the Theories of Galvanic Electricity. Manchester Memoirs, vol. ii. p. 293. Nicholson's Journal, vol. xxxv. p. 259.

Mr. Singer* has the candour to say, "to me the supposition appears to increase the difficulty, for it infers a series of decompositions and recompositions, of which we have no proof; and it does not seem probable that such phenomena should occur *without producing some apparent motion, or change in the interposed fluid.*"

Dr. Bostock's interpretation of the difficulty was, "that the electric fluid united with the hydrogen, with which it formed an *invisible* combination, and passed through the fluid to the negative wire, and that the electric fluid then entered the wire, and deposited the hydrogen, *when it became visible.*"†

To these philosophical ingenuities I must beg leave to add Dr. Prout's explanation of the theory of the constitution of water, and which is as follows:—"One volume of oxygen has contributed to form two volumes of water, which two volumes of water must consist of twice the number of *self-repulsive molecules* contained in the one volume of oxygen; yet every one of these molecules must contain oxygen because oxygen is an essential element of water; it follows therefore, *irresistibly, that every self-repulsive molecule of oxygen has been divided into two*, and consequently must have consisted of at least two elementary molecules, *somehow or other* associated so as to have formed only one self repulsive molecule."

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* Singer's Elements of Electricity, p. 380.

† Nicholson's Journal, 4to. vol. iv. p. 257.

The Doctor then observes, that "as we cannot admit the division of an ultimate molecule or atom, we must, of course, conclude, *that the molecules of oxygen and hydrogen must each of them contain at least three component or sub-molecules*."

Now this last ingenious reasoning, evidently the result of more than one arduous lucubration, is to meet *another* formidable difficulty resulting from Lavoisier's Theory, and which is no less than this, viz.—"that three volumes of the gases, (two of hydrogen, and one of oxygen) are necessary to form one atom of water, whereas, when this atom of water is brought into a gaseous state, *i. e.* when it is converted into steam, it only occupies the space of two volumes;" *so that the space of one of these volumes is mysteriously and miraculously annihilated.* I will not stop to inquire whether this fact alone ought not to be enough to overturn the theory?

Suffice to say, that it was to close up, as artistically as possible, this little chink in the theory, and through which the LIGHT so provokingly *peeped*, that the brains of poor Dr. Prout were thus grievously taxed in his Bridgewater Treatise.*

"*RISUM TENEATIS?*"

It has been said that "the errors of great men are the consolation of the dunces;" and it is most certain that our modern philosophers must now descend from that lofty wisdom, which would impart the secret of the MOST HIGH in the formation of water, and modestly taking leave of the "MAGNIFICENT FABRIC,"* condescend to correct the ignorance which so long mistook a compound for an element, and so absurdly contrived to make a chaos of one of the most useful and beautiful creations in nature.

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* See Bridgewater Treatise by Dr. Prout, pp. 123 to 126. Dr. Wollaston exclaimed, upon quoting a similar kind of argument, "*Strange influence of preconceived theory—blunting the sense, and stultifying the reason!*"

There is such a manifest deficiency of solid argument in all that Lavoisier says about the composition of water, that the more I read and consider what he has written in his Memoir, the more I am astonished that even men of ordinary perceptions could have been cajoled (I cannot conscientiously say reasoned) into this theory; and much more so that such a theory should have passed current with the *elite* of men of science. There must have reigned some extraordinary influenza in the intellectual regions which suspended the excrcise of ordinary intelligence. It can be no otherwise accounted for.

I only request the reader to look into Mr. Watt's Correspondence, and read Lavoisier and Monge's Memoir.†

In speaking of the water produced by the conflagration of the two airs, hydrogen and oxygen, they say that the "matter, therefore, of the heat and light" (which escapes during the operation) "has alone the property of passing through the pores of the vessels;"* "the two airs, which are ponderable bodies, could not have disappeared; they could not be destroyed."

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* See Lord Brougham's Life of Lavoisier.

† Pages 173 to 176.

Again, (they say), "The matter of the heat and light, which is disengaged in great abundance, possessed no sensible weight, which in fact might be taken for granted; so we concluded that water is not a simple substance, and that it is composed weight by weight of INFLAMMABLE and VITAL AIR."

Now what extraordinary reasons are these for establishing the composition of water! Two gases are put together, which are called principles, *i. e.* elements—they are set on fire and explode, and then give out a vast body of heat and light, which escape through the pores of the vessels; and then, because the gaseous matters have disappeared, and water is found in the vessel, it is concluded *per saltum*, that water is a compound of these two principles.† Why, in the first place, is there any

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[*] Sir H. Davy, reasoning from the presumed decomposition of water, was led to the conclusion, "that we had *not* discovered any elementary or indestructible principle." See Davy's Elements of Chem. Phil. vol. i. 182. N.B. It seems to me highly probable that even the *electric fluid* is the *inflammable principle* in a combined form.

[†] THE ACIDITY (the invariable acidity) *of the water produced by the deflagration of the oxygen and hydrogen* must not be overlooked. It was the great argument of Priestley against Lavoisier's water theory.

Upon looking over Mr. Cavendish's celebrated paper (Philos. Trans. for 1844, reprinted in Watt's Correspond. p. 111) in which ground for calling these gases, principles, *i. e.* elements?

he gives his experiment of the production of water by mixing 19,500 measures of oxygen with 37,000 measures of inflammable air, we find him to state that the water produced *was sensibly acid.* We also find him to say, that suspecting the purity of the hydrogen, he changed it by using that obtained from different substances, but still the water was acid. That at length he obtained his hydrogen from plants, but that *an acidity of the nitrous kind was still found in the taste of the water;* and that it appeared that "*the less the air was phlogisticated the more acid it was;*"* but that when the proportion of inflammable air was much greater, the standard being $\frac{1}{10}$, pure water was procured. But he says that when inflammable and *common air* are used, the "*condensed liquor is not acid.*"

Now, upon examining Dr. Priestley's experiments, (Doctrine of Water Refuted, pp. 47-50), we find him alluding to an observation of Dr. Maclean's, of the extensive scale and superior accuracy of the French experiments, with regard to the *water produced, and of its purity;* when Dr. Priestley exclaims, "When I can produce but a few drops *of a strong acid, and as often as I please, from the very same materials from which I am told I ought to get only pure water,* what is it to me whether they produce gallons?" He then adds, that while the greatest attention was paid by him to the glasses, and that his gases, he is sure, were *purer* than any the French chemists had made, "I got drops of *a stronger acid than can be procured by air,* LESS PURE." In a subsequent passage, he says, "From the *simplicity of my apparatus no other substances can possibly mix with the airs; and then I explode the whole at once by an electric spark.* After this, I presently find the result by examining the liquor that is drained from the vessel. *Though I have not gallons of this liquor, I have some ounces, which no anti-phlo-*

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* "Less phlogisticated," means, the greater the proportion of oxygen.

If they are elements, whence comes the inflammable matter and the light? Is it not conjoined with one or other of the gases? Is it not, then, a necessary consequence, that to one or other, or both of those gaseous matters, is combined an inflam-

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gistian would care to drink. Will Dr. Maclean," he adds, "say that my process is less accurate than that of the French chemist, *because it can be finished in less than five minutes, and theirs requires the assiduous attention of some days? I can procure water free from acidity,* as well as they can," he says; "*but then the air is impure,*" *i. e.* contains less oxygen.

Now it is to be remarked that you have here two of the ablest chemists, whose probity will not be for a moment questioned, asserting that *not pure but acid water results from the combustion of the gases in the proportions in which it is insisted by the French chemists that pure water is produced.* So, again, we find Monge to say distinctly, that the water has an *empyreumatic taste that water gains in distillation.* So much for pure water! —See Watt's Corresp. p. 213.

Now, when we find that metals will yield to the water a phlogistic matter, and we see therefore the greatest probability that hydrogen is a compound of such phlogiston and water, it follows, that when this hydrogen is exploded in oxygen gas, it necessarily renders up its *water, which* then absorbs the oxygen. It is then evident, that as oxygen is allowed by Lavoisier to be the generator of acidity, the water must be more or less acid, according to the quantity of oxygen used in the operation. Is not this in accordance with what Cavendish and Priestley say—that the water is always acid with a certain quantity of oxygen, but *not so with common air?*

The arguments I have adduced appear to me so invincible, that I have declined introducing this, though a strong objection, in the body of the work, at the same time that it might be considered inexcusable to omit it altogether.

mable principle, and that such is their state before they are ignited? Can this inflammable principle be fairly attributed to both the gases? Certainly not, as one of them, oxygen, is not inflammable, though improperly called a supporter of combustion. The inflammable principle must therefore be combined with the other gaseous matter, viz. the hydrogen, and which is found and admitted to be a highly inflammable matter, and therefore called by Lavoisier and Monge, "*le principe inflammable aqueux.*" This being indisputable, to which gas must be attributed the abundance of heat and light which escaped through the pores of the vessel at the time of the deflagration of these gases? Clearly, to the hydrogen. Then this same hydrogen being a substance or matter combined with an inflammable principle, parts with this inflammable principle in the operation in which water is produced. What right is there then for saying, in the first place, that water is the combination of the two gases, when *non constat*, but that the inflammable principle might have been combined with the very water *which you pretend to have formed*, and which has now lost its inflammable principle? And, in the second place, when the matter so left, viz. the water, is, at all events, only the result of the combination of the two gases *after* the inflammable principle has departed from them, what possible right can there be for saying that the *water so left* is the "RESERVOIR OF COMBUSTIBLES," as Lavoisier calls it?

But the wise disciples of Lavoisier will say, "This difficulty is removed by our other experiment, in which we decompose water, as we there reproduce the hydrogen, which proves that the inflammable principle must be derived from the water."

Short-sighted philosophers.

"Revant toujours et ne pensant point!"*

Why, in order to decompose the water (as you call it), and obtain again this hydrogen, are you not obliged to call in aid an inflammable substance, viz. either a metal or pure electricity? Can you get your inflammable hydrogen without it, as Mr. Grove imagined, and as the golden medal would infer?

The foregoing six paragraphs are alone sufficient to send this impotent theory to "its last account," so manifest are its irredeemable sins.

I will now venture to ask "the Committee of Chemistry," of the Royal Society, not whether they continue to believe in the "decomposition of water," but whether they can conscientiously justify themselves for having given credence to a theory so capable of being overthrown by a *process* consisting of a few days reading, a little examination, and a moderate portion of common sense.

"RISU SOLVUNTUR TABULÆ!"

That in any enlightened era, under any circumstances, a theory so improbable, supported by hypotheses so irrational, should have survived three-fourths of a century, and after that long period for discussion, examination, and reflection, should have received the solemn sanction of the ROYAL SOCIETY, are matters which must cast a deep shade on our modern philosophers,* and prove the wonder and amazement of succeeding ages!

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* Rousseau.

> "We grieved, we bled, *we never blushed before!*"

I cannot conclude this part of my Tract without recording a most remarkable circumstance, namely, that notwithstanding both Cavendish and Watt were of opinion that the "inflammable air" (hydrogen) *was composed of a small quantity of water*† *and much "phlogiston," or "elementary heat,"* (an opinion which they each reiterated again and again in their writings, and which even formed the subject of the last communication of Cavendish relating to hydrogen gas) it never once occurred to either of them that on the combustion of the hydrogen and oxygen gases, the former gas might be *decomposed*, and that the elementary heat which escaped might have left its watery base behind, and which would then necessarily absorb the oxygen, and thus form the acidulated water found in the vessel at the end of the process, instead of concluding, as they did, that the gases had *united* to form the water; an inference which was negatived, first by the *explosion*, which ensues upon the combustion of the gases, and which denotes a separation of molecules; and 2ndly, from the *escape* of the phlogistic matter accompanied with heat and light, and which was confirmatory of such separation. I think it must be admitted that this oversight is not the least extraordinary feature in connexion with this delusive theory. Our ancient philosophers would have exclaimed, "QUI VULT DECEPI, DECIPIATUR."

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* It will be said that I am not justified in passing this sweeping censure upon "philosophers" generally, and that instead of using the generic term, I ought to have confined myself to the class of chemical philosophers. But when I see divines, astronomers, literary men, legislators, legists, physicians, &c. (all of them men preeminent in their respective callings) enter the arena of discussion for the purpose of awarding the palm of victory to the discoverer of this notable theory, am I not bound to believe that these celebrities would not have exerted so enthusiastically their intellectual powers to do justice to a discovery of the TRUTH and merit whereof they had not previously inquired and felt themselves fully satisfied?

† This small quantity of water contained in the hydrogen accounts for the little water produced by Cavendish's and Lavoisier's process, as we have seen how few are the grains of water obtained from enormous quantities of the gases.

A contrary, but equally fatal misconception, seized upon Sir Humphry Davy when he concluded that his current of electric or galvanic fluid had *decomposed* the alkaline matters, soda and potash, instead of the fact that he had *united* this phlogistic matter to the alkaline bases, and thus formed his two fleeting metals, "sodium and potassium." But how extremely improbable it was that these transitory metals should be the bases of alcalis, seeing that they became decomposed with the first drop of water, while the alcalis have such an affinity for that element that they seize it with avidity from the atmosphere.

Again, what evidence was there of the *fiery decomposition* of these metals in the alcalis, and which must have taken place if they had formed the basis of those matters.

It is then but too manifest that we have committed egregious blunders—that our scientific sins have been many and great; but whether attributable to our want of sense, or the non-exercise of it, is an enigma I shall not stop to inquire into. One thing is however certain, viz. that there can be no signal folly without an appropriate expiation. Thus then pronounces RETRIBUTIVE JUSTICE. "You must recommence your chemical studies, and as a preliminary step, apply Chemist Scheele's 'fiery commotion' to all chemical books which have appeared since the days of Lavoisier, taking especial care to include his famous 'MEMOIRE' in such conflagration: and let Mr. Brande and Dr. Faraday see to the prompt execution of this my decree!"

SIC VOLO, SIC JUBEO!!!

THE

COOLING PROPERTY OF HYDROGEN.

"Felix qui potuit rerum cognoscere causas."

Mr. Professor Grove (the Author of the memorable paper upon water), at the meeting of the "British Association for the advancement of Science," at Swansea, in August, 1848 (some months after the publication of the first edition of the present tract), surpassed Lavoisier himself, by the communication of a paper on the *peculiar cooling* property of Hydrogen gas in respect of metals; and which the Professor illustrated by shewing that a coil of metallic wire ignited by means of a current of electric or galvanic fluid, lost its heat upon being immersed in that gas.

This paper, (under the auspices, no doubt, of Dr. Faraday, who must have been in exstacies at the discovery of the *natural* connexion which was found to exist between *water and hydrogen gas,* in consequence of the latter's *refrigerating* qualities,) would be no doubt transmitted without delay to the Royal Society by the Professor, whose mind, it may be conceived, would be beaming with delight with the anticipation of another golden medal.

Dr. Johnson has said "who forsakes the probable, may easily find the marvellous." It might, however, have been expected that a distinguished Professor who was or had been a Member of the Council of the Royal Society, before he announced so unexpected and astounding a discovery, would have paused a little, in order to examine more attentively the experiment, and see whether it would not admit of some more rational conclusion; and, had he so done, and the prevailing love of mystery and paradox had not got the better of the Professor's judgment, he would possibly have hit upon the true solution, and which is as follows.*

A metallic wire when it receives a strong current of electricity from the voltaic battery, is ignited in consequence of the accumulation and commotion of this phlogistic matter at the extremity of the wire where it is making an effort to pass off, and which the atmosphere that surrounds the wire does not assist, as it is a bad conductor of electricity: but hydrogen gas is, on the contrary, a good conductor, and consequently when the wire thus ignited, is plunged into hydrogen gas, the latter instantly conducts or abstracts the electric fluid from every portion of the wire, and the cause of the heat being thus removed, the necessary result is the cooling of the wire as shewn in Mr. Grove's experiment.

Chemists of all climes have been most censurably neglectful with regard to hydrogen gas. Notwithstanding its discoverer, Mr. Cavendish, stated that from his experiments he believed it to be a compound of phlogiston and water, and that both Dr. Priestley and Watt, from their experiments, perfectly agreed with him in that opinion, chemists have neither investigated the experiments of these philosophers, nor examined the matter themselves, but have preferred at once the adoption of an opinion the very acme of absurdity; or to use the mildest phrase, *the highest of all improbabilities*, viz., that a "phlogistic matter,"—"elementary heat,"—which the "inflammable air" was said to be, or allowed to contain, could be converted into, or become water.

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* See note, p. 107.

Our ancient critics in such a case would certainly have applied the old proverb,

> "Qui Deus vult perdere prius dementat."

HEAT AND COMBUSTION.

> "*Ignis ubique latet, naturam amplectitur omnem,*
> *Cuncta parit, renovat, dividit, unit, alit.*"
>
> See Lord Brougham's Life of Voltaire.

It may, perhaps, be not without advantage to chemistry, to communicate my ideas upon the above two subjects as they present themselves to my mind after an attentive examination of existing experiments, and the doctrine founded upon them.

I have *no new experiments to offer.* I have already said in my former tracts, that *I never made any either in chemistry or electricity.* This admission has drawn upon me a coarse philippic *from a professional* SAGE, and the awful charge of "*ignorantia crassa, &c., &c.*;"[*] and to which I must be allowed to reply, that it *may* possibly be found more beneficial to science that we should *understand existing experiments* ere " we fly to others that we know not of." I have the consolation, too, of not being singular in this opinion, as it was expressed by a *very acute, though not an*

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[*] It is the old maxim, "*Audacter calumniare semper aliquid hæret,*" and becomes " wise men " in their pursuit of truth.

infallible philosopher, some sixty years ago; and if it were a pertinent one at that period, it will be hardly thought less so at the present time.*

In speaking of combustion, I adopt Dr. Thomas Thomson's definition, viz. "a total change in the nature of combustible bodies, accompanied by the copious emission of heat and light." The Dr. blames Lavoisier and his followers, for calling *the union of a body with oxygen*, combustion, as that often happens, he says, without any extrication of heat and light. He also observes, "that Lavoisier's system of combustion did *not* satisfactorily account for *heat and light evolved during combustion;* and *that these phenomena were in a great measure overlooked by him*"—he might indeed have added, intentionally overlooked, as not being explicable upon his theory.

The union of oxygen with a body, without the extrication of heat and light (says Dr. Thomson), is very different from its union when accompanied by it, both in the phenomena and the product.

Lavoisier, he adds, accounted for heat and light, by saying, "*that they were combined with the oxygen gas*, and were separated from it when the combustion took place,

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* See the Dedication to this Tract, and the notes thereto.

IF MEN OF SCIENCE, who pursue the study of chemistry and natural philosophy *professionally*, experimented for the purpose of developing any *preconceived* principle or notion derived from their reading or observation, their experiments would be better analysed and understood. Instead of this, a precocious love of distinction too often converts them into scientific gamesters, and they merely dash from experiment to experiment in search of a lucky hit. The consequence of this is, that many a valuable experiment is passed over uncomprehended, whilst the only gain to society is the occasional discovery of some amusing experiment in electricity, optics, &c. which, when the blaze of its singularity has had its hour, sinks into insignificance, and is no more remembered. This seems to me a very minor, if not worthless celebrity, founded upon the mere "shreds and patches" of science.—See the 2nd note to the "Dedication," *anteà*.

when the oxygen, ceasing to be in its gaseous state, united itself to the combustible body."[*] But what experiment, I ask, ever proved that *heat and light are combined with oxygen,* to justify Lavoisier in making the statement?

But when Lavoisier talks about the oxygen *ceasing to be in the gaseous state,* he assumes that the oxygen is some matter *chemically* combined with a phlogistic matter which renders it gaseous; in which case oxygen gas cannot be an element, as we know that external or ordinary heat, though it will produce a vapour, cannot produce a gas, which remains *permanently* in a gaseous state, and does not depend, like the vapour, upon external heat for its support.

The whole hypothesis is, then, *a gratuitous assumption.*

Now I think I have shewn to those who are open to conviction—

1st. That metals contain a phlogistic matter; and that this same matter is common to all combustible bodies, whether solid or fluid; and that it is the combination of bodies with this phlogistic matter which constitutes their combustibility.

2ndly. I have also shewn that *hydrogen gas* will reconstruct metals from their calces, and give to the latter the metallic lustre and other properties of a metal; and that the same effect will be produced by *electric sparks,* or *an electric current;* and from which circumstance it necessarily follows, that hydrogen and the electric fluid contain the same phlogistic principle.[†]

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[*] Thomson's Elem. of Chem. 5th edit. vol. 1, p. 141. Lord Brougham in his life of Lavoisier, says, his theory consists in supposing that all combustion like all calcination, is produced by the union of oxygen with the body burnt or calcined; and that the gas, which in calcination only gives out *its heat and light* slowly and imperceptibly, (unless when this operation is performed *very rapidly,*) *in combustion* gives out that heat quickly and sensibly.

[†] Watt, says Dr. Black, made it appear *probable* that metals owe their malleability and ductility to latent heat.—Watt's Cor. p. 154. But Sir H.

Now it is established, that when hydrogen and oxygen gas are mixed in a close vessel, and fired by the addition of external heat; *i. e.*, either by means of an electric spark, a burning taper, &c. a violent explosion is the consequence, accompanied with a very considerable evolution of heat. It may be here remarked, that in all cases of combustion *where heat and light are produced, extraneous heat has been invariably applied* to the combustible substance, the oxygen of the atmosphere not being sufficient to produce such phenomena without the aid of excitation, and which such additional heat effects.

I observe that Scheele, who had paid much attention to this subject, says, "*to every combustible body a certain quantity of heat must be communicated, in order to set it in a fiery commotion.*"* The same thing occurs in the calcination of metals, an increase of heat being necessary to assist the oxygen in overcoming and driving away the phlogistic matter contained in them, before the oxygen will be imbibed. Lavoisier and Meusnier say, "*in fact, the calcinable metals, in like manner with the combustibles, do not become sensibly alterable by oxygen, but by the aid af a high temperature.*"†

It therefore follows that three things are necessary to produce combustion with heat and light, viz. a combustible body, oxygen, and a certain degree of additional heat.

Now, it is said by Lavoisier, and it is the universal belief of chemists *without exception*, that when the hydrogen and oxygen gases are thus deflagrated, *they unite chemically*,‡

Davy, and before him, Beccaria, had *proved* that the electric fluid revived the metal in all its lustre.

* Scheele on Air and Fire, p. 111.

† See their Memoir, Watt's Corresp. p. 154.

‡ It is to this error that may be attributed the misconception about the decomposition of water. In fact, these gases do *not unite*, and never will unite under any circumstances, as they are irreconcilably opponent principles.

and that it is by this *chemical combination* that heat, light, and water are produced; the heat and light, be it remembered, *escaping*, and leaving only the water behind. This heat and light Lavoisier attributes to the *oxygen*, as we have seen; but why he did so, except to throw a mystery about his theory, it is difficult to imagine. The hydrogen was indisputably proved *to be an inflammable body*, and which he himself always asserted; why, then, not have attributed to this matter the light and heat, instead of attaching it to the oxygen, of which *there was no evidence whatever*, of its being an inflammable substance?

Now, *this affinity between the hydrogen and the oxygen*, and which is the very basis of the water theory, I have not been able to discover from any one experiment. On the contrary, it appears to me *that they are substances which never in any case chemically unite*, and that when any attempt is made to conjoin them (as by heat or electricity) the result is a *decomposition of the hydrogen*, when its phlogistic base escapes, and its watery base remains which absorbs the whole or greatest part of the oxygen; the amount of such absorption depending (of course) upon the quantity present of the latter gas.

It seems to me that the whole doctrine of combustion is contained in that simple operation, the deflagration of hydrogen, in oxygen gas, when the phlogistic principle or electric matter is liberated and escapes.*

The *explosion*, which was so strangely taken as a proof of fraternity or union, is the mere separation of the electric matter from the water with which it was previously combined.

* If Ovid, when he says "*caret ignibus æther*," discriminated between common air and oxygen, we might note him down as a great chemist.—Metam. lib. xi. v. 520.

With regard to *light*, its greater or less brilliancy may be owing to the nature of the body with which the phlogistic matter is united, and which, being decomposed at the time the phlogiston escapes, the latter, in rapidly

That hydrogen and oxygen *never* exist in combination, appears, I think, distinctly, on an attentive investigation of the experiments. For instance, take the metallic calces, and you find that the earthy base has an equal affinity for both the gases, but that it unites with each singly, and never together; that when the one gas enters the base, the other invariably quits it. We see that when the metal is in its perfect state, *i. e.* when the earthy base contains the phlogistic matter, and that by artificial means you deprive it of that matter, the earth then unites immediately with the oxygen, and forms the calx or oxide: and that when you afterwards drive away the oxygen, the earth will immediately unite with the phlogistic principle of the hydrogen, and restore the metal; but that these two gases never combine at the same time with the earth.

Mr. Watt, in speaking of the supposed attraction which exists between hydrogen and oxygen, says, " but in order to unite them *the inflammable air must be made red hot, or* nearly so; *but why this attraction does not take place* (he adds) *to the same degree in the common temperature of the atmosphere is a question* I am not able to solve." He also says, " that it is well known that *these two airs will remain for years in a closed vessel, at the common heat of the atmosphere, without suffering any change*; the mixture being at

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passing off, may carry with it some imperceptibly minute particles in a state of ignition. To these invisible particles we may, therefore, be indebted for a *vivid light*. I think this the more probable, as we find that although the heat produced by the combustion of hydrogen is *intense*, yet the light is barely perceptible, and which arises, I would say, from the base of the phlogiston in this case being water, and therefore contributing no sparks.

This hypothesis is supported by another remarkable circumstance, viz., that the " *colour of the light* depends in all cases (says Dr. Thomson) upon the nature of the combustible body;" and which proves that a connexion must exist between the phlogiston and its base after the former has escaped, or become liberated. See also Singer's Elem. of Elect. p. 91.

the expiration of that time *as capable of deflagration* as when first shut up.*

This all seems confirmatory of their *not* uniting; and I can find no unequivocal instance of any chemical union of the two gases; *but quite the reverse.*

They seem to me to be ANTAGONISTIC PRINCIPLES, and to separate with violence when any means are used to cause their union with the same base.

In taking this view of the subject, and which I think all the experiments support, I find that combustion is nothing more than *the phlogistic matter quitting the body* with which it was combined; and that the vivacity of the combustion, and of its being accompanied with great heat and light, is wholly caused by the *quantity of phlogistic matter evolved,* and the *rapidity* with which it quits its base; and that the *intensity* of these phenomena (heat and light) are regulated by that circumstance.†

There is another proof of hydrogen and oxygen not uniting, viz., *that the former must have left the combustible body before the latter will unite with it.* It is for this reason that when a body is saturated with oxygen it is no longer combustible, as the saturation of the latter gas shews that all the phlogistic matter has quitted the body.‡ This construction is in accord *with our intelligence,* and the mind feels satisfied with it; whereas the doctrine of Lavoisier, which makes the combustibility to depend upon the oxygen, and afterwards accounts for a body being *no longer combustible* by its being *saturated with the very matter* which caused its inflammability, seems to me to be anything but satisfactory, if not absolutely inconsistent. Sir H. Davy was not able to make any advance in the doctrine of combustion. He could not reconcile himself to Lavoisier's theory, but could offer nothing better in its place, than motion.

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* Watt's Corresp. p. 83. These airs, therefore, (as Watt calls them) require to be excited by extraneous or ordinary heat, before the water of the hydrogen gas will part with its electric base, on parting with which it will imbibe the oxygen, and become acid.

† See, as to Light, note pp. 110, 111.

‡ This agrees with Stahl's ideas, who says, " All combustibles contain phlogiston, and combustion is the separation and *dissipation* of this principle; heat and light being properties of phlogiston."—Thomson's Chem. Essays, 5th Edit. vol. 1. p. 135.

Dr. Thomas Thomson, indeed, distinctly admits that this theory of Lavoisier's only applies to the oxygen when in a *gaseous state;* but that, when both the supporter and the combustible are found in a solid state, as in the materials composing gunpowder, it is wholly inapplicable. The principle of Lavoisier is, that when a body is united to oxygen, combustion cannot exist, because in such body *combustion has already taken place.* Now, says Dr. Thomson, gunpowder (which, when kindled, burns with great rapidity in close vessels, or under an exhausted receiver), is composed of nitre, charcoal, and sulphur; the two last being combustibles; whilst the first is the *oxygen*, nitre being composed of nitric acid and potash. But here the oxygen (says Thomson) is not only *already combined with azote, but forms a component part of a solid;* yet a great quantity of caloric and light is emitted during the combustion, *and almost the whole product of the combustion is in the state of gas.* This appears, he says, *doubly inconsistent* with the theory, for the caloric and light must be supposed to be emitted from a solid body *during its conversion into gas,* which *ought to want more caloric and light for its existence in its gaseous state than the solid itself contained.*[*]

The theory, then, is evidently insupportable; but I am still quite at a loss to conjecture upon what grounds these chemists have pretended to establish the *strong affinity* which they insist exists between the hydrogen and oxygen gases; and which doctrine seems to have been *universally admitted.*

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[*] Elements of Chem. vol. 1. p. 142.

On the explosion of the two gases, we find the heat and light *to escape,* accompanied with such an *expansion* of the gaseous matters as to require a glass vessel *of half an inch in thickness to resist the shock !**

Is this enormous expansion, I would ask, any proof of a chemical *combination or condensation* of the gases into a liquid state?

When this same experiment, or a similar one, is made by introducing the hydrogen or oxygen gases into a strong glass tube, closed at one end, and shutting the other with a cork, through which an electric spark is passed, and which is succeeded by a *loud detonation, the expulsion of the cork takes place with great violence.* Is there here, again, any reason for concluding that a *combination* of the gases has been effected?

When the explosion of gunpowder takes place with the *evolution of these two gases,* is there any ground for inferring their union? Is there not, on the contrary, the most decisive evidence of their separation?

Mr. James Watt found that the *dilatation* of steam by *heat* might be carried *to* 18,000 *times the original bulk of the water without any sign of decomposition.*† Now, do we infer from this state of vapour, a combination *or condensation* of the particles of water? On what principle, then, has it been *dogmatically assumed,* that after the explosion of these gases they unite and condense into a liquid state? *We know, further, that hydrogen gas cannot be condensed into a liquid by any possible pressure;* that has been tried in vain.* Indeed, the very notion of compressing an igneous principle is ridiculous.

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* Singer's Elem. of Elect. p. 192.

† See Thomson's El. Chem. vol. i. p. 180.

All the experiments that I have met with, appear to me to prove *indisputably* that hydrogen (or rather phlogiston) and oxygen are altogether *opposite principles*, and are *intended to produce opposite results*, and that they cannot by their nature unite, but must invariably remain separate from, and independent of each other.

Now, as the electric fluid either is, or contains the same phlogistic principle which exists in hydrogen, and as combustion is the separation of this phlogistic matter, with more or less velocity, from the body with which it is united, it follows that the electric matter, or the phlogistic principle which it contains, *is the cause of heat.* It is also clear that this phlogistic matter (and which accords with Dr. Black's doctrine of *latent heat*†), *lies dormant* in the body with which it is united *until* it meets *with its antagonistic principle*, and that the two gases *are excited*, or *put in commotion*, by an electric spark, or burning taper, &c. *when the phlogistic principle rushes from its base*, and the latter unites with the oxygen, which takes the former's place.

When the combustible is a compound of two substances only, as in metals, the oxygen combines wholly with the earth, and then becomes necessarily an *incombustible body*.‡

There is, however, another experiment besides that of the metals, which appears to me to prove conclusively *that hydrogen and oxygen gases have no affinity for each other*,

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* Thomson on Light and Heat, p. 205.

† Watt found *the latent heat in steam* to be *enormous*, and this accounts for the *vast quantity of electricity which steam gives out from a metallic boiler.* —See Thomson on Light and Heat, p. 200.

‡ Lavoisier, in his Experiments on Phosphorus and Sulphur, shews that, when burning, they absorb oxygen, *i. e.* oxygen supplies the place of the phlogistic matter which, at the same time, quits these substances

and do not chemically unite; and which is that of the combustion of the diamond.

The diamond, for instance, is suspended in a closed glass vessel filled with oxygen gas, when, either by sufficiently elevating the temperature, or by passing into the matter the electric spark, combustion of the diamond takes place, and which finally is wholly decomposed.

The diamond, as Sir Isaac Newton predicted of it, is the *most combustible* body known. It necessarily then contains the greatest quantity of phlogistic matter. This phlogistic matter is united to pure carbon; these two matters constituting, as it is admitted, the substance called a diamond.

Now, upon firing the diamond in oxygen gas, what is the result? First, the phlogistic matter with its heat and light escape from the vessel which cannot contain it; such heat and light continuing as long as any phlogiston passes rapidly off, or, in other words, until the whole diamond is consumed.

No *solid* residual matter is found in the vessel at the termination of the combustion, but *merely carbonic acid gas.*

Now, I say, that as the *whole* of the matters in the vessel consisted of carbon, oxygen and a phlogistic matter (and which chemists say is the hydrogen), if such an affinity as is pretended had existed between the hydrogen and the oxygen, they must have united and formed water instead of carbonic acid gas according to the theory.*

It is evident, however, *that such phlogistic matter quitted the vase*, and that it must have been pure phlogiston which was combined with the carbon, and *not hydrogen,* as no water is left behind, none being found in the vessel. You have, then, in this experiment all the elements of combustion, *viz.* a combustible body composed of carbon and the phlogiston, and you have the oxygen, and by bringing to these matters the additional heat *necessary* to put them (as Scheele says) in "fiery commotion," you effect a combustion, when the phlogistic matter flies off, yielding its place to the oxygen, and in passing off in sufficient quantity and rapidity, is accompanied with intense heat and light.

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* With regard to the supposed affinity of oxygen for carbon, see page 78.

The diamond, though possessing this large share of phlogistic matter, gives no indication of heat by the thermometer, such heat (as Dr. Black says) being latent, and requiring to be excited or set in motion by means of its opposing element, oxygen, and the aid of superadded electricity, or of external ordinary heat.

That this phlogistic matter does not in its ordinary state possess the power of communicating heat by mere contact, is, I think, made evident by passing an electric spark on to, or taking it from the living body, or even by giving the shock; neither of which produces such a sensation. To produce such an effect, it seems necessary that the phlogiston should be concentrated in, or combined with some substance, and that it should be liberated therefrom by being put into the fiery commotion already noticed.

In metals that oxydate in the open air, and from which the phlogistic matter must consequently escape, no sensible heat, and still less what is ordinarily understood by the word combustion, is discovered; nor is light manifested in such operation.

It should therefore appear that the phlogistic matter must pass off with continued velocity (and which is the result of the commotion caused by the aid of oxygen and extraneous or ordinary fire) to produce heat; and with still greater rapidity and quantity to produce light.* With respect to the existence of light in a phosphorescent state, it is not necessary to discuss that question here, as it seems not to be connected with the subject of combustion.

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* This would seem to coincide with the doctrine, that heat and light are modifications of the same matter, which is capable of giving or assuming either modification (says Dr. Thomas Thomson) *by means not known.*—Elem. of Chem. 5 Edit. 1 vol. p. 133. See note as to vivid light, ante p. 110.

There exists another proof of the want of that affinity, which it is insisted exists between hydrogen and oxygen, and that is that Sir H. Davy found that "when one volume of oxygen and two of hydrogen *are mixed with other gases, they lose the quality of exploding;*" a circumstance not explicable on the supposition of any such affinity.*

I think, therefore, it is evident that the hydrogen (or rather phlogiston) and oxygen are with respect to each other, *incompatible substances;* and also that certain bodies receive with *indifference* either of these matters; the rule being, that when the one element quits, the other takes possession; *but that they never co-exist in the same body.*

In concluding these crude observations upon combustion, I would suggest whether this displacement of hydrogen for oxygen, and *vice versâ,* which we find to take place with the metallic earths, does not arise from the preponderance *in quantity* of the displacing element? For instance, that when the oxygen and hydrogen are put into "fiery commotion," (as Scheele says), whether the circumstance of the one or the other uniting with the metallic earth does not depend upon which of the two elements† is present *in the greatest quantity?*

Dr. Priestley and his contemporaries knew very little of electricity, and it is only since his time that an indistinct notion has sometimes been entertained of its being the principle of *latent heat.* Thus we find that Berzelius was disposed to found a theory of combustion upon the existence of *two electricities.** Sir H. Davy seemed also to hold that chemical affinity was identical with electrical attractions; and that bodies which unite chemically are differently electrified.

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* See Watt's Correspond. Appendix, p. 77, where Mr. James Watt makes the same remark.

† I use the word "element," to hydrogen for convenience only.

These unsettled notions of the agency of electricity were carried much further by Dr. Wollaston, who attributed to the *oxidation of metals,* the evolution of electricity, and the extrication of the hydrogen from water to the same matter; Dr. Thomas Thomson too, in speaking of the production of fire by the friction of two pieces of dry wood, says that the *caloric* evolved in this case *is not due to the combination of oxygen* (and in proof whereof he gives an experiment of the Genevese professor Pictet), *nor from any other discoverable cause;* and the Doctor then intimates, that it may possibly be traced to *electricity,* which he adds, *fuses the most refractory metals, and sets fire to combustible bodies, obviously* (he remarks) *proving that electricity evolves both heat and light.*

It appears most singular that after these near approaches to TRUTH, no one should have advanced a few steps further and have saluted (respectfully) the coy maiden herself—that it should never have occurred to any one of these philosophers† that the phlogistic principle or latent heat, and the electric spark were identified; that they were nothing different but in name. And yet how could this important fact be doubted, the moment it was established that the calces of metals, might be *revived either by hydrogen gas, or by the electric spark?*

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* I have shewn in this and my former tract that *two electricities do not exist.* I have also shewn that the electric fluid and the magnetic fluid (a modification as it should seem of the former) *continually* traverse our globe in a northern direction, and which gives the appearance of polarity or attractive power to the needle by carrying it mechanically in that direction; and which theory accounts for all the different phenomena which have been hitherto attributed to some occult or mysterious attraction between the needle and the North Pole, one of the many absurdities that we have from time to time adopted.

† "Though reading," says Lord Bolingbroke, "makes a scholar, yet every scholar is not a philosopher, nor every philosopher a wise man."—Cooke's Memoirs of Lord Bolingbroke, vol. 2. p. 195. Lord Bolingbroke should have given us his interpretation of a philosopher.

Even if this could not dispel all doubt, was it not clear that the ductility and malleability of metals are due to latent heat? Is it not notorious that it is to this cause that iron owes its different degrees of ductility? from the cast iron, when it is most brittle, and contains the least quantity of latent heat, to the wrought iron, when it is most charged with that matter, and when it is found to be the most malleable? Do we not oxidate the metals, and thereby liberate the electricity which we find, and make use of, in the galvanic battery? Did not Watt ascertain the *vast amount of latent heat* which exists in steam? and do we not find that when this matter enters the atmosphere and becomes condensed, that it parts with volumes of this same latent heat? And of what do we find this matter to consist? *of electricity in copious streams!*

What, then, is this idle pretension of the non-existence of *a phlogistic matter*, when we find all nature to be teeming with it? And yet, in defiance of experience and common-sense, we are wedded to a theory which is *ex facie*, absurd, and which upon examination is not found to have one redeeming quality; a theory which makes it a chemical heresy to talk of phlogiston, while it is found to be one of the *most essential* of elements.

But this doctrine, like most absolute and exclusive creeds, owes its origin less to error than to vanity, and will undoubtedly terminate, as it merits, in utter disgrace.

It may not be inopportune to conclude this part of my Tract with the following notice:—

A SYNOPSIS OF LAVOISIER'S SYSTEM OF CHEMISTRY, INCLUDING THAT OF COMBUSTION.

Lord Brougham (who always treats his subjects with the ability of a philosopher) in his Life of Lavoisier, has well described this system, by stating that it was grounded on the *subversion* of the doctrine of Stahl, who taught that substances when in combustion *part with a phlogistic matter.* Lavoisier "in his important doctrine of calcination," (says Lord Brougham) "called by him Oxidation, *overthrew* Stahl's doctrine, and shewed that *metals do not part with anything* in passing from the reguline (metallic) state; that on the contrary, they absorb and fix oxygen gas. The importance of this blow," continues Lord Brougham, "induced him to follow it up *by denying that combustion is a process which evolves any component part from bodies.*"

Having thus given the outline of Lavoisier's system from Lord Brougham, I will enter somewhat more fully into the detail of what it consists, and shew wherein its fallacy lies.

Stahl, (but still more clearly, I think, Scheele), had stated that a metal was a compound of two things—an earth *sui generis*, and a phlogistic matter; and as a metal is a combustible body, it followed that combustion was the result of putting the phlogistic matter in motion, and making it quit the body with which it was united. *That this phlogistic matter or elementary heat, was the electric fluid, had, however, occurred to no one.* Some time afterwards Padre Beccaria made known an experiment by which he distinctly proved that electric matter would restore a metallic *oxide* to its reguline state; but he did not know that in bringing back the oxide to its metallic state, he had *deoxidated** the earth and replaced the oxygen by the electric matter; the fact being, that oxidation is the state of the earth when combined with oxygen, and the metallic state, when the same earth, *instead* of the oxygen, is united to the electric matter.

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* Priestley proved this by his experiments, though it might be supposed that it was Sir Humphry Davy's discovery, as Lord Brougham says, "Davy never

The error of Lavoisier (if he was really deceived, which is very questionable) lay in assuming that when the metal had become oxidated, it had merely imbibed oxygen, and *without parting with any matter;* and he enforced this argument by shewing that the metal had increased in weight. Now this, though indisputable evidence that the metal had united itself to some other matter which was ponderable, did not prove that the metal had not at the same time parted with some matter *not ponderable.*

Now the electric or phlogistic matter which quits the metal when the oxygen enters, is precisely in this condition, as it is an imponderable substance.

Combustion or calcination, then being the result of the electric or phlogistic matter's quitting the metal, it follows that the intensity of the combustion, or that state of it which is accompanied with sensible heat and light, must depend upon the quantity of phlogiston or electric matter evolved in a given time; so that the process of combustion will admit of graduation from the most intense heat to one insensible or imperceptible.

The fact of an electric matter existing in a metal admits of no dispute, as the *revivification* of metals when in a state of oxidation by a stream of electric matter was established by Beccaria and Priestley (as I have already observed), and is also to be found in Sir Humphry Davy's experiments.

Now when the revivification of a metal takes place in a

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complained of their (French Institute) interference during his illness with *the process of deoxygenation by means of galvanic action.*"—See Lord Brougham's Life of Sir Humphry Davy.

closed vessel, nothing is found in the vessel at the end of the process but the revived *metal* and pure oxygen, and which is demonstrative of the fact that the particular earth combined with the electric matter, constitutes the reguline or metallic state; and that the earth will only imbibe the oxygen when this phlogistic matter quits it, and when it will then become an oxide. If oxygen then is the sole cause of combustion (according to Lavoisier), that phenomenon ought to subsist as long as the oxygen remains united to the body. This, however, we know is not so, but just the contrary, as an *incombustible* body, *par excellence,* is a body combined with oxygen.

To get rid of this difficulty,—(which Lavoisier thought a little too much to be received by any class of chemists) he said that when a matter absorbed the oxygen combustion *had* taken place. Then let me ask what is combustion upon this theory? It clearly does not consist in the oxygen, nor is it in the substance with which the oxygen is united, because Lavoisier says *that the substance parts with nothing.* Then combustion is necessarily made the result of motion, or the act of union between the substance and the oxygen. But then *why does motion require oxygen* to produce combustion?

Now this is the theory which so delights Lord Brougham, and draws from him the highest encomiums on the genius of Lavoisier.—"The *overthrow* of the phlogistic theory, and the *happy generalization upon the combination of bodies* (oxidation) make him regarded," says his Lordship, "*as the first reformer of chemical science*, and the principal founder of that MAGNIFICENT FABRIC which now fills so ample a space in the eye of every *student of nature.*"* Most strange that clever men should be so misled!

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* Life of Lavoisier by Lord Brougham.

It is pretty clear that Lavoisier, like many other scientific men, was not over anxious about the fame of others, but extremely interested in his own, and finding that Stahl's celebrity rested on a theory, which, though *highly probable*, turned upon a principle (phlogiston or elementary heat) which could not then be exhibited in a concrete shape (and which matter he, Lavoisier, could make nothing of), he availed himself of his discovery that oxygen* united with metals in their calcination (oxidation), to set up a theory of which he might say he had got rid of phlogiston. This then is the "*magnificent fabric*," which, as we have seen, falls upon the first assault, and proves to be as worthless an edifice as any philosopher could well build up.

It is evident from the experiments of the two gases (oxygen and hydrogen) on metals, that a base which unites with the one gas has an *equal affinity* for the other. It is also to be collected, that the two gases are *opponent principles*—invariable antagonists, and that when one of the gases is united to a base, it cannot be removed but by the superior force of its antagonist, *i.e.* by an overpowering quantity of the opponent gas. It is in this way that a metal is first oxidated, and afterwards deoxidated.

These noble elements, Phlogiston and Oxygen, (the former being the electric base of the hydrogen) must however be *roused* for the combat by the stimulus of external heat, when victory most wisely declares herself in favour of the champion who appears in the field with the strongest force.

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* Oxygen was discovered by Priestley.

A REVIEW

OF THE

CLAIMS TO THE DISCOVERY OF THAT REMARKABLE DELUSION

"THE DECOMPOSITION OF WATER."

"*E tenebris tantis clarum extollere lumen.*"

Berthollet, and the other chemical philosophers who made their report of the Examination of Mr. Kirwan's Essays, say:—" One of the parts of the modern doctrine, the *most solidly established*, is the formation, the decomposition, and re-composition of water. *And how can we doubt it*, when we see that in burning together 15 grains of inflammable air, and 85 of vital air, we obtain exactly 100 grains of water, *in which, by decomposition, we find again the same principles, and in the same proportions.** *If we doubt of a* TRUTH *established by experiments so simple and palpable, there would be nothing certain in natural philosophy!*" Natural philosophy then is assuredly in a most hopeless state!

It has been said, " that opinion is stronger than truth," but however this may sometimes be, *it is not more certain* that autumnal snows will vanish before the " glorious orb of day," than that this *most solid* THEORY *will melt away before this little tract*, and like a midsummer night's dream leave nought behind but a passing smile *to mark the frailty of human reason !*

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* These able men were so blinded by their doctrine as not to see that this *reproduction* of their " inflammable air" ruined their theory, as it forcibly drove them to seek for the phlogistic matter which had escaped when (according to this "most solidly established theory,") they had formed the water, and which, therefore, would be necessarily *minus* a matter indispensable to the formation of this same "inflammable air."

Before I enter upon this inquiry, which will be very short, I feel desirous of doing justice to M. Arago, the author of the *Eloge* of the late Mr. James Watt, by recording a very *remarkable and shrewd question* put by him when discussing some ambiguity in Watt's expressions about phlogiston :—"*Even at this day,*" says M. Arago, "*have we mathematically demonstrated that hydrogen is an elementary body, or that it is not, as Watt and Cavendish supposed, the combination of a radical and a little water?*"* Mathematically prove hydrogen to be an element ! I would tell M. Arago that there is a much greater chance of his squaring the circle.

With regard, then, to the discussions in the Edinburgh and Quarterly Reviews, and those of M. Arago's, Lord Brougham's, &c. it would seem that the same want of discernment has prevailed in the investigation of the claims of Mr. Cavendish and Mr. Watt to this imaginary discovery, as in that of the theory itself. The question, in fact, notwithstanding all this galaxy of science and criticism, has never been perfectly understood ; and it is this obscurity in the minds of the critics which occasions the uncertain and unsatisfactory state in which the inquiry is left.

In order, then, to arrive at a clear comprehension of the question, I will first state what it is *not.*

1. The question, then, is not whether inflammable air, burnt in or with common air, produces water ; as that was discovered by Macquer in 1776,† who, by placing a china saucer in a flame of hydrogen in the open air, found a residuum of both *soot and water.*

2. The question is not, whether the same matters produce

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* See Corresp. of Watt, p. 253.

† See his "Dictionnaire de Chimie," pub. in 1778. Mr. Cavendish says (Phil. Trans. for 1784, p. 126), that it was the deposit of *dew* observed by Warltire which induced him to make his experiments with the gases.

water when burnt in a closed vessel; as that was ascertained by Warltire, in a copper vessel, in 1781.

3. The question is not, whether the same matters deflagrated by an electric spark in a *closed glass vessel* produce water; as that was effected by Warltire in Dr. Priestley's presence also in 1781.

4. The question is not *even whether water is produced by oxygen and hydrogen in a closed glass vessel, and deflagrated by the electric spark;* as we shall immediately see that this was discovered by Dr. Priestley, after the last experiment of Warltire.

To prevent any doubt being entertained upon the latter point, I will adduce the most undeniable authority, *viz.*, the letter of Mr. Watt to Dr. Black, of the 11th April, 1783;* wherein he says, speaking of Dr. Priestley's experiment, that Priestley reports that, "*when quite dry pure inflammable air (hydrogen) and quite dry pure dephlogisticated air (oxygen) are fired by the electric spark in a close glass vessel, he finds after the vessel is cold, a quantity of water adhering to the vessel, equal, or very nearly equal, to the weight of the whole air.*" He afterwards adds:—"*in the deflagration of the inflammatory and dephlogisticated airs, the airs* UNITE *with violence*† *and become red hot, and on cooling totally disappear: the only fixed matter which remains is water; and water, light, and heat are all the productions.*"

We have therefore here the *whole* experiment; for as to the proportions of the gases, that was a subject which would be inquired into and resolved as a necessary and unavoidable consequence of the experiment itself; and in this opinion I am also supported by another unquestionable authority, *viz.*, Lavoisier himself, who says in his Memoir to the Academy of Sciences, of 1783, p. 479 :*—" Since water is PROVED to be composed of inflammable and vital air, *it is clear that Mr. Priestley found water without knowing it.*"

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* See Watt's Corresp. p. 19.

† This fatal and erroneous notion of the union of the gases caused all the misconception about the formation of water, as I have more than once pointed out.

Now no notice whatever is taken of any similar cotemporary or antecedent experiment by Mr. Cavendish, and therefore the priority of the discovery of the result produced by the burning together of vital and inflammable airs (without regarding in what proportions) is unquestionably due to Dr. Priestley.†

But notwithstanding this, and that Lavoisier gives him the credit of anteriority, neither he, nor any other critic, has ventured to attribute to Dr. Priestley the *discovery* of the pretended theory of the decomposition of water. Why? for the reason alleged by Lavoisier, that Priestley *did not know* of the discovery he had made. Nay, I will go further than that, he never believed (to his credit) that either that experiment, or the *converse* of it, *i. e.* when the water is supposed to be decomposed, established the theory.

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* Watt's Corresp. p. 185.

† It is possible that Priestley might have taken his idea of this experiment from Scheele. Dr. Thomas Thomson's account of it is this : *He says that hydrogen and oxygen remain together unaltered unless fired by an electric spark or a burning taper, and then they produce a violent explosion.* If, he adds, one part in bulk of oxygen and two of hydrogen are thus exploded, they leave no visible residuum, the vessel which contained them being *filled* with water. This important experiment, he further adds, was made by Scheele, *but for want of a proper apparatus he was unable to draw the proper consequences.* Mr. Cavendish ascertained that the water left by the combustion of the gases *equalled their weight.* Hence he concluded (says Dr. Thomson) *that the two gases had combined and formed the water. This was confirmed, he adds, by Lavoisier and his friends.*—Elements of Chem. vol. i. p. 224.

Now as to the water equalling in weight the two gases, the same thing would happen whether the water be the result of the union of the gases, or whether the hydrogen be only decomposed ; as in the latter case the water left behind would absorb the oxygen, making the weight exactly the same in both cases.

N.B. Thomson says not a word of Mr. Watt, in respect of this discovery, *though his countryman.*

The whole question then is clearly this; who first *propounded** the theory "that water is a compound and that it is composed of the *elements* hydrogen and oxygen united by deflagration."

Now, most unquestionably, this theory emanated from Lavoisier, and from no other person; for who had previously said the hydrogen was an element? and which unless assumed, the theory fell to the ground. I will challenge any English or Scotch critic to produce one single *unequivocal declaration* from either Cavendish or Watt's writings of such a theory, or from which such a theory is to be clearly and indisputably inferred.

But the complete answer to any such unequivocal sentence being found in anything that Cavendish or Watt has written, is the *indisputable* fact, that the notion they each entertained of the *supposed* constituents of water, rendered it *impossible* for them to have adopted the idea of such theory *prior* to its announcement by Lavoisier, when they possibly might have changed their minds.

For in the latest writings of Cavendish upon this subject, in 1784, what expressions do we find him to use? "Dephlogisticated air is in reality nothing *but dephlogisticated water, or water deprived of its phlogiston*, or in other words, *that water consists of dephlogisticated air united to phlogiston.*"†

But when it is asked what is phlogiston, what does Mr. Cavendish say? why this, "that inflammable air is (as he repeats it to be several times in his paper) *either phlogisticated water, or else pure phlogiston.*" Observe the expression, "phlogisticated water," and which was a notion so firmly impressed (and I will add, well founded too) upon his mind, that he registered it in his note affixed to his celebrated paper, upon which his claim to priority is grounded, and which I have before cited.*

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* I omit the word "conceived" advisedly.

† See his Memoir in the Phil. Trans. for 1784, and in Watt's Corresp. p. 136.

Now, what is the unavoidable meaning conveyed by phlogisticated water, but this, (and which Mr. Watt expressed most clearly,) *an inflammable matter combined with water?* So that, in fact, he believed *one of the supposed constituents to contain the very matter it is proposed by the theory to form.*

I am not here garbling the expressions of Mr. Cavendish; I have infinitely too much respect for his high character and talents to wish to make one uncandid observation upon what he has written; but it is not possible to read his paper with attention, without seeing his opinion remained wholly unshaken as to hydrogen being such a compound as that I have just stated.

This definition of hydrogen, without noticing his equally equivocal definition of oxygen (viz., "*dephlogisticated air is in reality nothing but dephlogisticated water*") renders it wholly impossible to attribute the invention of the theory to Mr. Cavendish.

Lord Brougham remarks, that it was *after Lavoisier's paper had arrived* that Mr. Cavendish, for the *first time*, distinctly states, as upon Lavoisier's hypothesis, that water consists of hydrogen and oxygen gases.† The observations I have made entirely coincide with this remark of Lord Brougham's, which proves that if Cavendish did ultimately adopt the theory of the decomposition of water, it was after Lavoisier's announcement of it.

Now, with regard to Mr. Watt, he is still further removed from the discovery, in consequence of his more explicit statement of what he considered hydrogen to be, and which is found in his letter to M. de Luc, of Nov. 26th, 1783,* in which he states: " *It has been known for some time that inflammable air contained much phlogiston;* and Dr. Priestley has found, by some experiments made lately, that it is *either* wholly pure phlogiston, or at least that it contains *no apparent mixture* of any other matter." " *In my opinion, however,* it contains *a small quantity of water and much elementary heat,*" and which is exactly the fact.

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* *Vide* antea, p. 24.

† Watt's Corresp. p. 247.

Then, again, in the fourth paragraph of his letter, Watt says, " For many years I have entertained an opinion *that air is a modification of water,*" &c. So, again, in his letter to Mr. Hamilton, of the 22nd April, 1783, he says, " *All substances which produce inflammable air, are substances which contain some* WATER *firmly united to them, and have some principle which is known to attract phlogiston strongly.*"†

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* See Phil. Trans. for 1784, p. 329 to 353, and Watt's Corresp. Appendix, p. 77.

† Watt's Corresp. p. 20. Lord Brougham, in his Life of Watt, tells us that, after fully considering all the experiments, writings, and dates relating to the water theory, " Watt was, *without doubt*, the person who first discovered the composition of water."

Now let us see the grounds upon which Lord Brougham arrives at this decided conclusion.

" Mr. Watt," says his Lordship, " in his letter of April, 1783, and in a passage of the letter of November, 1783, stated ' *that inflammable air contains a small quantity of water, and much elementary heat,*' " and which Cavendish and Watt both called phlogiston. Upon this Lord Brougham observes, " we may remark there is the same uncertainty or vagueness introduced into Mr. Watt's theory which we before observed in Mr. Cavendish's, by the use of phlogiston (elementary heat) without exactly defining it. Mr. Cavendish leaves it uncertain whether or not he meant by phlogiston *simply inflammable air, though he inclines rather to call inflammable air water united with phlogiston.* It must be admitted," continues Lord Brougham, " that such

It is not necessary to quote anything further from Watt's papers, as the above extracts shew his deliberate and invariable opinion upon the nature and composition of inflammable air; and which renders it impossible that the theory of the composition of water should be claimed by him, any more than by Cavendish.

It must be borne in mind, that the theory in question is constituted of this predication, viz., that hydrogen and oxygen *are elementary principles,* and that, combined together, they form water; ergo, that water is composed of those two elements.

Now, how can it be affirmed of any person that he has promulgated such a theory, when he tells you distinctly, *that he believes one of the constituents to be itself a compound, and to contain the very matter which it is proposed to make?*

Is there then any philosopher, who, before Lavoisier, said that hydrogen and oxygen (or inflammable and dephlogisticated air) *were elements*, and that by combination they formed the (compound) water? It is quite clear that this question must be answered in the negative, and conse-

expressions on the part of *both of these great men betoken a certain hesitation respecting the theory of the composition of water.*"

Now, surely, from such premises, it is a startling conclusion for so able a mathematician and critic to come to, that "Watt was, *without doubt*, the first discoverer of the composition of water."

Why if Watt believed, or held the opinion, that the "inflammable air" (hydrogen) contained water (and upon which there can be no doubt), he must have seen that the water, which was said to be formed, was not merely *in posse,* but actually *in esse* in the hydrogen, of which it was a component part, and that it would merely require the "inflammable air" to be *decomposed* to come at the water *in specie.* How, then, is it possible to make Watt the discoverer *at all* of the composition of water?

His Lordship's predilection for Scotland, and desire to uphold the fame of its philosophers, must have been great indeed to carry him so far beyond the boundary (as it appears to me) of sound reasoning.

quently thet Lavoisier has no competitor for the water theory.

Lord Brougham, however, in his historical note appended to M. Arago's "Eloge of Watt," in discussing the inference to be drawn from the equivocal and ambiguous phrase, that "*oxygen is water deprived of phlogiston*," used by Cavendish and Watt, observes, that there is *no substantial difference, perhaps*, between that expression and Lavoisier's hypothesis that *water consists of oxygen and hydrogen gas*.* I am a little surprised at such a suggestion from such a person. I would illustrate what appears to me a marked distinction between them, by the following parallel: Suppose three persons to be discoursing together about colours, and that the first interlocutor should affirm that green was a compound colour, and that it was composed of two elementary colours, blue and yellow: and that the second interlocutor should say that *blue was green deprived of yellow;* and the third, that *yellow was green deprived of blue;* which of the three interlocutors would be presumed to understand the subject? In this predicament, it appears to me, stand Lavoisier, Cavendish, and Watt. The first interlocutor is decided, and gives a clear idea of his subject; whilst the two others have only imperfect and confused notions about it; and therefore their expressions are vague and indefinite.

Now, in drawing this conclusion, which I think clear and satisfactory, in favour of Lavoisier, I by no means intend to attribute to him the *first conception* of the theory, as well as the *promulgation* of it.

It is impossible not to feel that Cavendish must have conceived the possibility of the theory before Lavoisier propounded it; *but he doubted, because he was startled and*

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* Watt's Corresp. p. 247.

puzzled with the hydrogen; and this admirably illustrates the distinctive character of the two nations.

The English philosopher contemplated the theory, and felt all the *eclat* that would attend the announcement of it; but he *lacked conviction*, and the high moral character of the English gentleman, the "*mens conscia recti*," preponderated over his vanity.* But the vanity of the eminent French philosopher was not to be controlled by any such conscientious scruple. He therefore seized upon the *delusive prey*, and with it gained a transitory fame, to terminate with the lasting reproach of having retrograded chemical science for more than half a century!

I was not aware that the palm of victory regarding this memorable discovery, had ever been disputed by other competitors than Mr. Cavendish and Lavoisier, until reading the articles in the Edinburgh Review and M. Arago's *Eloge* of the late Mr. Watt, and afterwards Lord Brougham's life of that philosopher.

In the correspondence, however, of Watt, we find a piece of evidence in favour of Mr. Cavendish (if the subject were one of serious import) which I should think it not a little difficult to overcome, or rather to be decisive of the question. In a note in *pencil*, made by Mr. James Watt on the copy of his letter to M. de Luc of the 21st Nov. 1783, he says, that "*he believes Mr. Cavendish was the first who discovered* that the combustion of dephlogisticated air (oxygen) and inflammable air (hydrogen) produced *moisture* on the sides of the glass in which they were found."†

Now, as it was well known to Mr. Watt that the mere production of water by such means had been first effected by Dr. Priestley, a fact which he mentions in the same letter, it is evident that he must have meant to say by his note, that Cavendish was the first person who had *drawn the inference* (which Dr. Priestley did not) that water was produced by the combination, and was a compound of those gases; and as the note in question seems to approach very nearly to that overpowering testimony which results from a man *in extremis* disburthening his conscience of a weight that presses uncomfortably upon it, it seems to me to be irresistible.

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* The late Dr. Arnold said, upon anotheroccasion, "a *thorough English gentleman,* manly and enlightened, is more, I believe, than Guizot and Sismondi could comprehend; it is a finer specimen of human nature than any other country, I believe, could furnish."—*See the Life and Correspondence of Dr. Thomas Arnold.* 1844.

† See Watt's Corresp. Appendix, p. 77.

That admirable sentiment of nationality, however—that deeply-rooted patriotism, which is found in the Scotchman, and which I could wish were as strongly marked in the English character, does not allow defeat to depend upon such a casualty, and Mr. James Watt's claim is strenuously insisted upon, however *he himself may have repudiated the honour.*

Though I must, therefore, most conscientiously doubt the justice of the claim, I cannot treat lightly the feeling which dictates it—a feeling which indisputably gives birth to strength of intellect and manly virtues. I trust, therefore, that this sentiment will continue with our very shrewd and tenacious neighbours, *until* they shall prove by *more satisfactory* evidence, that Minerva had a sister by a Caledonian lady, and shall place her statue in the *other parthenon* of "Modern Athens;" and *until* the English language shall have ceded to the more euphonic and graceful twin-Grecian, or Gaelic dialect;* *desiderata*, whose accomplishment will probably long demand all the energies of "all the Talents."

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* The ancient writers take notice of this northern "concord of sweet sounds."—"*In borealibus quoque trans Humberum Eboracique finibus simul canendo symphoniaca utuntur harmonia.*"—"vocum modulando varietatibus *demulcente pariter et delectante.*"—Gerald Cambr. cap. xiii.

The extinction of the English name in the refulgence of "Modern Athens,"

seems to have been foreseen by the Latin poet—"*Inde Caledonio velata* † *Britannia Monstro.*"—Claud. xxii. 247.

Also Casaubon, the son, speaking of the English language, says, "*Scotica lingua Anglicâ hodierna purior.*"—*Mer. Casaubon, de vet. ling. Ang.*

[†] From "*velo.*" Edit. emend.

THE STUDY OF MATHEMATICS.*

It has been said, "that no great genius would be a mathematician." On the other hand it is insisted, "that the study of mathematics has an advantage over all others in sharpening the perceptive faculties, and invigorating the judgment."

Upon the first proposition I offer no opinion. Upon the last I must be allowed to say, that it is at least, very doubtful. The study savours too much of the mechanical to produce such a result.

An acute critic has said, "If there be any psychological fact established beyond dispute, it is, that mere mathematicians are commonly inferior to men possessing equally natural powers who have varied the range of their ideas." If this be admitted as an axiom, it may still have no great bearing upon the proposition in question.

Have we, however, any strong evidence in support of it? That some celebrated men have been at the same time good mathematicians, only proves that a clear head will excel upon all subjects to which it applies its powers. But does the mathematical mind draw conclusions from premises generally with more facility and greater accuracy than other men? That is the vital question. Now applying it to the theory discussed in this tract—"the Water Theory," what is the result?

[*] Referred to, page 21.

Mr. Cavendish was an able mathematician, and many of the contemporaries of Lavoisier were signally distinguished for their mathematical acquirements—viz. Meusnier, Monge, Laplace, &c., and who all supported his theory; and in later times we have M. Poisson and Dr. Wollaston,—and yet none of these gifted men detected the obvious and fatal objection to the theory which I have pointed out. Again, not to dwell upon M. Arago, the distinguished Astronomer and Author of the Memoir of Watt, we have at the present moment an eminent man, ennobled for his talents, and who has applied himself especially to the theory in question: I mean Lord Brougham. Now this distinguished Peer, (who has warmly expressed his indignation at the impertinence of Voltaire for calling Leibnitz and Descartes charlatans*), is not only familiar with the "integral" and the "differential or fluxional calculus" ("the torch with which," it is said, "Newton surveyed the heavens,") but is master, as he tells us,† of the "lost Porisms of Euclid," and by which there is a possibility of finding that case "in which a determinate problem becomes indeterminate," and admits an *infinity* of solutions, *all of which are given by the statement of the case*." And yet thus armed with all the panoply (I apprehend) which mathematics can furnish, his Lordship did not discover the objection any more than his predecessor.

The "mathematical torch" therefore either does not shed that flood of light over the mind which is contended for, or when these eminent mathematicians explored the realms of chemistry they must have met with some chemical agent which neutralized its power, or altogether annihilated its virtue. At all events we have here no satisfactory evidence of the value of mathematical studies.

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* See his Lordship's Life of Voltaire.

† See his Lordship's Life of Simson.

A SUMMARY

OF A FORMER TRACT,

DEMONSTRATING THE ERRORS

IN THE EXISTING SYSTEMS OF

ELECTRICITY AND MAGNETISM,

AND

POINTING OUT THE CAUSE OF THE POLARITY OF THE NEEDLE.

THERE seems every reason to suppose that the galvanic and magnetic fluids are only modifications of the electric fluid, as all the experiments tend to establish that identity.

Sir H. Davy's opinion was, "that the *electric and galvanic fluids are the same,* the apparent difference depending on the intensity and quantity."

I should rather conclude, however, that the difference is not merely from the cause Davy states, but from a combination of the electric fluid with some foreign matter.

The source of the electric fluid cannot now admit of a doubt. It is evident from what has been already stated, that this fluid is one of the elements of metals from which it is liberated on their being decomposed or oxidated; and it is equally clear that the quantity of the electricity, evolved in a given time, depends upon the rapidity with which the metals are oxidated.

Davy says, "that atmospheric air or oxygen, or nitrous or muriatic acid in solution in the water, will produce the oxidation of the metals, (*but not pure water alone*), and that the galvanic phenomena are in proportion to the *rapidity* with which the metals (zinc particularly), are oxidated." This he repeats in another place, remarking, "that the plates will act no longer than the water between them holds *some oxygen** in solution."

With regard to the nature of the electric fluid, Franklin treated it as having two properties, the one positive and the other negative.

The French chemists believe that there are two distinct fluids, and we appear now to have adopted this conclusion.†

From the mysterious mode of action (as it was supposed) of this matter, Davy thought "the phenomena of electricity, were produced by a highly volatile *fluid* or *fluids* of which the particles are *repulsive with respect to each other, and attractive of the particles of matter!*"

Dr. Prout, in his Bridgewater Treatise, has given the prevailing opinion with respect to electricity and the cause of its phenomena. "It seems," he says, "to be *satisfactorily proved* that the phenomena of electricity depend upon *two energies* mutually existing throughout nature in a state of equilibrium, in which state their peculiar powers are not perceptible,—that these energies can be *partially separated* and kept asunder."

"That if two bodies charged *in excess with the same energy*, be brought into the vicinity of each other, they mutually repel each other; whilst two bodies *charged with the two different energies* mutually attract each other."*

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* See Dr. Paris's Life of Davy, vol. i. p. 246. How improbable then is the supposition that oxygen can be one of the elements of water, when it is the absence of oxygen which prevents the pure water form acting on the metallic plates.

† If there be but one fluid, said the French philosophers, (who had lost their faith in miracles,) how inconceivable it is that it should possess two opposite properties, one attractive and the other repellant, and that these two properties should be found in the smallest imaginable portion of the fluid.

But this, with deference to Dr. Prout, is only solving the "*ignotum per ignotius.*"

Priestley had before observed, that Mr. Symner had produced a number of experiments to shew the existence of "*two* electric fluids, *not independent, but always co-existent,* and yet *counteracting one another.*"†

Now all these theories, which carry with them absurdities at which our sense revolts, are erroneous. There are no two fluids; there are no two such things as positive and negative electricity; and the supposed attractive and repulsive properties have no existence.

The electric fluid, (which there can be little doubt acts an important part in the arrangement of the atoms of all substances,)‡ as well as the magnetic fluid *is constantly moving* upon our globe in the direction of the North Pole; and it is from this simple fact alone, as I shall presently shew, that flow all the phenomena of electricity and magnetism which have puzzled the greatest men of modern times.

Every doubt, indeed, upon this point vanishes the moment we find that if the needle be so magnetised as to destroy the current, the *polarity does not exist.*§

All combustible substances (I will here observe) are bases combined with more or less of the electric fluid, according to their peculiar constitution; and all excess of this fluid existing on any matter whatever, (that is so much as is not *chemically* combined with the substance) passes from such matter into the general current; and this passage of the fluid necessarily takes place with more or less facility according as the body it is upon conducts it with more or less facility.

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* See 8th Bridgewater Treatise, p. 36.—*If instead of assuming an improbability to be a fact, and then endeavouring to account for it,* philosophers had used their "energies" to see whether such appearances might not be illusory, they would very probably have escaped the rocks and shoals on which they have been wrecked.

† Priestley's Hist. of Electricity.

‡ Davy says—"I have shewn that chemical attractions may be *exalted, modified, and destroyed by changes in the electric state of bodies.*"

§ See p. 144.

"Common electricity," says Davy, "is *excited upon non*-conductors, and is readily carried off by conductors and imperfect conductors."

It would have been more correct to have said "that if non-conducting bodies be excited by friction, they will acquire electric matter from the atmosphere," which may then be carried off as he has stated.

Now, there are three sorts of substances, which act upon the electric matter in a particular manner, viz. conductors, *par excellence*, imperfect conductors, and non-conductors.

Thus if we want to obtain a portion of the electric fluid in circulation for any purpose, we use a non-conducting body, which on being excited by friction is found to be *charged* with the fluid, which may then, by a conductor, be passed on to the Leyden jar, or any other apparatus adapted for the deposit of the fluid.

It may be remarked here that a peculiar property is met with in the electric fluid, viz. that of its *perpetual motion*, unless counteracted.

"Electricity," says a diligent observer, "is the only element which is *constantly in motion*, unless restrained; the only one, therefore, that may be said to possess force."*

Now if the electric fluid be in perpetual motion unless restrained, and if such motion be constantly in the direction of the North Pole, it is evident that such fluid, and every modification of it (such as the magnetic fluid) which may exist on a conducting body, must have a tendency to join and be carried with the general fluid. These simple data being premised, (and they are not controvertible) the consequences are self-evident.

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* See an article by Dr. Bence Jones in the "Lancet of July 10, 1847," p. 32.

For instance, suppose a circular brass rod of any given dimensions, *a*——————*b*, to be charged with the common electric fluid, by introducing such fluid at the end, *b*. Now this metal being an excellent conductor, it is clear that this fluid in order to pass with the general circulating fluid, will proceed instanter upon the rod from *b*, to the other end, *a*; and it is equally clear, that as the atmosphere is not nearly so rapid a conductor as the metal, the fluid, when it reaches *a*, must remain accumulated there, until by the slow conducting power of the air it is carried off to join the general current.

We will now advance in *medias res*, and come at once to the cause of the polarity of the magnetic needle, a problem which might have remained unsolved for ages, from the strong disposition of the mind for what is mysterious—the omne ignotum pro magnifico.*

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* It is a remarkable circumstance that the predilection for the marvellous is to be found in all grades of society.

The ablest mathematicians, and the closest logicians are tinctured with it as well as the uninstructed. About four years since I heard the celebrated French Astronomer and perpetual Secretary of the Institute, M. Arago, deliver a Lecture on Astronomy at Paris, when incidently speaking of vision, he said that we saw objects double, and those in an inverted position, defects which we *corrected* by experience, or as he expressed it, *by education*. This doctrine, he told us, was founded upon experiments made by Chesselden and others upon persons born blind, and who, when they had acquired their sight, had their vision thus imperfect, and that all philosophers had adopted it.

Now, instead of this strange, and I must be permitted to add, absurd doctrine (which I believe is not exclusively M. Arago's but is to be found in the books,) what does *common sense* say upon the subject but this? that the

For instance, let us take a small steel bar, similar to what we see in a Dolland's pocket mariner's compass *a*——————————*b*. Now, if we wish to magnetize this bar, so that the North Pole shall be at the end *a*, we must pass the magnet along the bar from *b* to *a*, and always repeat the operation in *that* direction. If on the contrary we desire the North Pole to be at *b*, we must pass the magnet the reverse way, or from *a* to *b*.

It is evident that in either way we produce a magnetic current, and therefore from whichever end this current escapes, that end (when the bar is suspended, so as to turn freely as in the compass), must point to the North Pole, such being the direction the fluid is travelling.

That this is the case is indisputable, when we look at the effect of opposing current to current, as in the following experiment.

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optic nerves of these patients from long want of use, did not perform, instanter, their functional duties, and that time was necessary to enable them to recover their primitive power.

Will any man say that this is not a clear and natural, and therefore a satisfactory reason ? *Why then resort unnecessarily to the marvellous ?*

We know that objects are inverted upon the retina ; but does it follow *as an inevitable consequence*, that the impression upon the mind is also inverted ? Then why gratuitously assert as a fact that which is contradicted from our earliest experience ?

So with infants who it is said have an illusory vision, and see objects double : may not this be satisfactorily accounted for, from their brain and nerves not having yet reached their maturity ?

Instead of drawing these *natural* inferences, the philosophy of the nineteenth century has preferred the gratuitous assumption, "that it has pleased the DEITY to endow us with *delusory* organs of vision, whose deceptions, however, by dint of education and experience we contrive to counteract."

When M. Arago had made this philosophic communication, (though not precisely in these words) the foreign SAVANTS and DIPLOMATES (who invariably honour his lectures with their presence) joined the rest of the audience in vehement applause.

"*Videbis fili mi, quam parva sapientia regitur mundus.*"

Cancell. Oxenstiern.

Let the bar of steel *a* ———————— *b* receive the magnetic fluid in the direction *b—a,* that is by passing the magnet several times from *b* to *a,* (when the North Pole, as we have seen, will be found at *a.*) Instead of now continuing the operation in the same direction, let the magnet be passed several times on the same bar from *a* to *b.* What follows? There will be now no polarity at either end of the bar when it is suspended as before; and, therefore, for the purpose of indicating the North Pole, the bar will be useless. Why is this? Because, as must be manifest, by thus reversing the currents we oppose the current *a* to the current *b,* and which two currents must consequently meet in some *intermediate* part of the bar, where an accumulation of fluid will be formed, and where the fluid must escape in the best way it can; for as neither current can return upon itself, so neither current can reach either extremity of the bar.

In this operation, therefore, the magnetic fluid will appear to be neutralized and its properties annihilated; an evident proof that without a current there can be no polarity.*

With regard to the phenomenon called the "Dipping of the Needle"—this may arise from some inequality on the earth's surface, by which the fluid takes a downward course, and in that direction carries the needle. Or there may be occasionally in the earth near the surface, large quantities of metallic strata through which the fluid may pass on its journey.*

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* That the North Pole is the effect of the *exit* of the fluid, and not of attraction, is evident from the following experiment:—Let the bar or needle *a*——*b* be magnetised by passing the magnet from *b* to *a,* when the North Pole will be found at *a.* Now, if this Pole depended upon attraction, inasmuch as in this case the attraction at the end *b* would be equal to that at *a,* the needle would necessarily remain stationary, and could not indicate either Pole.

The power then called attraction is evidently confined to the tenacity with which the particles of the fluid adhere together, and in that state pass from conductor to conductor, thus causing an apparent cementing together of the two conductors, and which attraction extends no further than where there is a communication between the particles, although that may not be evident to our senses.

This tenacity of the magnetic fluid must be owing to the combination of the electric fluid with some matter at present unknown.

We will now proceed to examine the supposed positive and negative properties of the common electric fluid, and in which the adhesion of the particles is not so great as in the magnetic fluid.

Suppose, then, two circular rods of brass of equal dimensions, viz.: (*a* (1) *b*) *a* (2) *b* and that each rod is charged with the common electric fluid, introduced at *b*. It is evident, that if these two rods, so charged, be kept isolated, the fluid will have accumulated at the end *a*, of each rod. Now then remark what follows. If I place the end *a*, of rod 2, in contact with the end *b* of the other rod, I shall form, as it were, but one conductor; and it is therefore evident, that the fluid which exists on the rod No. 2, will quit that rod, and pass on to join the fluid of rod No. 1, at *a;* and if I charge a thousand rods, and place them together under the same circumstances, so as to form but one conductor, all the fluid will pass on to the first rod, on its way to join the general current.

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* On referring to my former tract, I perceive that I made some observations upon the horse-shoe magnet which are incorrect, and which arose from my having at the moment thought that the action of this magnet differed from the ordinary one.

Again, if I place the ends *b* of the two rods so charged together, no phenomena will take place, it being evident that in this position of the rods there can be neither attraction nor repulsion, nor any other apparent property manifested, as the fluids are now travelling in different directions, and wholly independent of each other's movements.

But suppose, instead of placing the end *a* of one rod to the end *b* of the other, (which produced the phenomenon of attraction), we oppose the ends *a* of the two rods, that is, place them nearly together, what must be the manifest result? It is clear that the two accumulated fluids will be now placed in a state of antagonism, and that, like two currents of water, or any other two fluids in motion, opposed to each other, instead of attraction, a violent commotion will take place, and that all matters found or placed within their sphere of action must be forcibly carried away. Now, this antagonism taking place, with the magnetic or the common electric fluid, produces the phenomenon called repulsion.

Thus, then, is explained all the mystery of attraction and repulsion, and of positive and negative properties, about which volumes have been written.

If, however, all the phenomena of electricity had been confined to these peculiarities, it is to be hoped, for the honour of science, and the acumen of its professors, that the occult nature of these operations would have been sooner brought to light.

It, however, happens that substances are met with upon which are found *only* positive electricity, as it is called; and upon others *only* negative electrity. Thus, then, it should appear as if there must be two fluids, as they are now apparently found detached.

Then, again, some substances will receive electricity from some bodies and refuse it from others, so that it is concluded that certain bodies will only receive a *peculiar* kind of electricity.

Now, all this is illusory, as will be readily understood and admitted upon examining a little farther into the nature of the different conducting bodies.

The common electric fluid may be collected, (as we have seen) and for some time retained, upon a non-conducting body, for instance, glass. Why ? because the fluid must remain on such a body in a comparatively motionless or stationary state—an uneasy state, to use a metaphor—a desire to leave the body and join a current of electricity, but an incapacity to do so, for want of a conducting medium. Suppose, then, the fluid to be now existing on such a surface.

Suppose again a *conducting* body, a metallic rod for instance, to be *charged* with the electric fluid, having received such fluid at *b*, and which has consequently passed on to *a*. Now, place the end *a*, of this rod (the positive end as it is called), where the fluid has concentrated itself, in contact with the fluid which is thus constrained on the non-conducting body. It is plain that in this case there can be no attraction—why? Because *both* the fluids are endeavouring to escape, and in this situation neither of them offers to the other any facility or means of so doing; on the contrary, the result of their approximation is to produce a scuffle between them to the advantage of neither; they are opposing each other. But instead of the positive end, *a*, place the negative end,* *b*, of this conductor on the non-conducting body, and you immediately discharge the latter of its electricity.

It is evident, that as the fluid on glass or any other non-conductor is struggling (as it were) to escape, such escape will be rendered more difficult by opposing it to the positive end of a conductor *charged* with other fluid; but that if the negative or *uncharged* end of the same conductor be offered to the fluid on the non-conducting body, the latter fluid will immediately attach itself to this uncharged end.

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* That is the end which the fluid has left.

Now to account for this result, instead of the above simple explanation, our scientific men have created two fluids, calling that upon the metallic rod the positive, and that upon the non-conducting body the negative fluid; and they have assumed from the above experiments that there exists a warm attachment (affinity) between the positive and negative families, while on the contrary these two mysterious fluids, as between themselves, that is when in their separate establishments and confined to their respective houses, are upon terms of utter enmity, and that nothing but discord reigns between them.

It is found again that the electric fluid upon two non-conducting substances will not unite; that when one non-conducting substance, charged with the electric fluid, is presented to another non-conducting body also charged, no sympathy takes place between them, but quite the contrary. I have already given the reason of this, namely, that both fluids are in an imprisoned state, their motion in a current being suspended; and as each fluid is seeking (if I may be at liberty to use this expression) to escape, it is evidently not the opposition or the union of the two fluids which can effect the object, but a conducting body, which neither of them possesses; and consequently placing the two non-conducting substances together only embarrasses their condition. The result then is, that a juxtaposition of two bodies under these circumstances produces no other phenomenon than repulsion.

Now there occurred a circumstance which enveloped this occult subject, (as it appeared to the great practitioners of electricity), in impenetrable obscurity, and called in vain for professional ingenuity to account for.

Dr. Faraday found that a metallic ball isolated—that is, suspended in the open air by a silken string,—would receive or be charged with the electric fluid (by induction, as it is called),* from the atmosphere.

This, according to what I have laid down, is an inevitable consequence. The fluid being always in circulation, and metal being a powerful conducting surface, the ball must in this case be charged with electricity.

But the surprise of Dr. Faraday arose from his finding, that notwithstanding the ball was of metal, and therefore the best of conductors, it was charged with the *negative* fluid; a discovery which must have caused him considerable astonishment.†

Now this again, according to the theory I have stated, is not only perfectly intelligible, but is also a necessary consequence; whereas, according to the theory of the books, it cannot be accounted for, and must have been in direct opposition to all Doctor Faraday's ideas on the subject.

To explain this apparent anomaly, I have already stated that a body is a non-conductor, or an extremely bad conductor, when the fluid rests upon it in a comparatively torpid or imprisoned state, the natural condition of the fluid being that of a current, which in this case of the ball, cannot exist. I must here observe, that to constitute a good conductor, it is not exclusively a metallic substance which makes it so, but also the *shape* of the metal, and which must be such as to facilitate the transit or passage of the electric fluid. It is for this reason that metallic *rods* are employed; it being evident that if the metal has either a circular or an oval shape the fluid will be dispersed over the entire plate of metal, which must be altogether destructive to the formation of the current. The more the positive end of the rod is pointed, the easier the fluid will necessarily pass.

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* This abstraction of electric fluid from the atmosphere is denominated now "induction." This tendency to neologisms is sheer vanity, and ought to be suppressed. The mind is already emasculated and overborne with nullities.

† See Dr. Faraday's paper in Sir David Brewster's Philos. Mag. of June, 1843, p. 478.

It is evident, therefore, that although metal is an excellent conductor, it may be so shaped as to have all the disadvantage of a non-conductor; and in that case, all the difference that can exist between a metal and a non-conducting body will be, that the metal will be highly charged with the electric fluid by induction (that is, by exposure to the atmosphere), whilst a non-conducting body will receive little or no electricity.

Now, when the metal is formed into a ball, it is precisely of the shape in which it must act as a non-conducting surface or body.

How can the electric fluid pass off from the metal in this globular shape, better than it could from any non-conducting surface? But the moment this same ball of metal is by the hammer elongated, and offers an extremity or end for the fluid to escape from, the metal will then be no longer negatively but positively charged by induction or exposure to the atmosphere.

It is clear from what has been stated that a ball of metal *cannot* be charged with positive electricity by any means; neither by induction nor by the electrical machine. Its form places it in the condition of a non-conducting substance, and no non-conducting matter can be charged with the so-called positive electricity. Positive electricity, be it remarked, is the fluid at liberty,*—viz.

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* Negative electricity is the same fluid, not moving in a current.

in motion as a current—and which it cannot be on a non-conducting surface, or on a metal of a form not allowing of a current.

However, to put this subject beyond the possibility of cavil, and to prove, incontrovertibly, that the ball is charged with the supposed negative fluid, solely from the cause I have mentioned, if when the ball is so charged, a short metallic rod $|^{a}_{b}$ be placed upon, or attached to the ball, with the upper end *a* pointed, so as to facilitate the passage of the fluid into the atmosphere, the fluid on the ball will be found immediately changed from a negative to a positive fluid; and it will be also found, that so long as the pointed rod remains affixed to the ball, it will be impossible to charge the latter, by induction or otherwise, with negative fluid.

It is evident that by attaching a rod of metal to the ball, a current will be aided; and it will be found that from that moment the fluid will resume its natural motion, and become, to all intents and purposes, according to the nomenclature of the day, a positive fluid.

I trust that I may now congratulate myself upon having satisfied the mind of the *most scrupulous philosopher;* and I trust, too, that I shall have relieved our ingenious and zealous "professors of natural philosophy" from the painful extra labour of drawing, perhaps, too largely, on their mental powers for *conscientious objections;* and to which irksome avocation they might have been driven, had the proofs and arguments been less overpowering than those which I have presented to their notice, and which will enable them, without hesitation, to bid a final adieu to the romantic notions of "self-repulsive molecules," and other miraculous atoms of scientific creation.

I will now say a few words respecting the *modus operandi* of the voltaic pile.

THE VOLTAIC PILE OR GALVANIC BATTERY, is an apparatus, as we know, formed of disks of metal which are kept wet by means of an acidulated water.

Metal, as we have seen, is composed of phlogiston or electric matter and an earth, *sui generis*, and that when acted upon by an acid, the electric matter is set free, and the earth at the same time unites with the oxygen of the acid, and form what is called an oxide.

The disks consist of two orders of metals, one of which is less easily oxidated than the other. The disk easily oxidated is followed by the one less so, and so on; and the consequence is that the easily oxidated disk when it touches the acidulated water, parts with its electric matter, while the following disk acts as a conductor of such matter, and by which means the electric fluid is passed along the battery or pile in a current, and which the operator can direct to whatever object he chooses.

Now as hydrogen is a combination of the electric fluid and water, it follows that the end of the battery from which this electric current escapes or passes (called by our scientific instructors the positive pole), must be that where the hydrogen is formed and consequently found; while the surplus oxygen liberated from the acidulated water (after furnishing the requisite quantity to oxidate the metallic earth as it parts with its phlogiston) will be necessarily found at the other extremity, or negative pole, as it is termed.

This is the whole mystery of the voltaic or galvanic battery.

THE MORALITY OF PHILOSOPHERS.

(*Note referred to p.* 10.)

Our savants are of opinion that morality and philosophy have no necessary connection; that they are in the condition of aliens to each other; that jealousy, manœuvre, and intrigue are not incompatible with the character of a philosopher. "Sir Humphry Davy," says Dr. Faraday, "smiled at my notion of the superior *moral feelings of philosophic men, and said he would leave me to the experience of a few years to set me right on that head*."—(See Dr. Faraday's Letter to Dr. Paris, of 23rd Dec^{r}., 1829, in the latter's Life of Sir H. Davy, vol. 2. p. 2.) And though this letter was written by Dr. Faraday 16 years after the interview to which it refers, he does not intimate that his own feelings or experience were in any manner at variance with the opinion Davy had formed.

Now, this libel on "philosphers" cannot be too soon, nor too severely reprobated.

The vast mistake which these savants have fallen into is in calling *themselves* "philosophers." The vanity of our nature has led them to imagine that facility in manipulation and dexterity in experiment, with energy of purpose, would make a philosopher, and they concluded that in their numerous rambling experiments, they were exploring the paths of science, when they were only wandering in the mazes of error.

As these savants have hitherto been merely roving about the environs of science, and are now unexpectedly conducted to the TEMPLE, they will allow me to counsel them for the future, to indulge less in experiment, *and more in reflection.*

One of the many advantages of this change of system will be its tendency to awaken their moral perceptions, and to enable them to discover that knowledge and morality are most nearly related—that science developes and expands, purifies and exalts the moral sentiments—that wisdom and virtue, *in fine*, are indissolubly united.

ADDENDA.

The author's Theory of the Phenomena of the Electric and Magnetic Fluids, and their mode of action, was communicated by him to the "Royal Society," from Aix-les-bains in Savoie, through their late esteemed Secretary, Dr. Roget, in August, 1834.

The Theory was referred by the Society to a Committee to report upon, but no report was ever made.

In December, 1834, the same theory was communicated by the author to the "French Institute," in a memoir presented by M. Arago, when the subject was referred to Messieurs Ampère and Becquerel, and subsequently to another Committee, but no report was made by either.

The author also communicated the Theory of the Magnetic Needle to the Board of Admiralty in February 1838.

Early in 1844, in consequence of seeing in the Philosophical Magazine of Sir David Brewster's, for June, 1843, Dr. Faraday's experiment noticed in the foregoing pages, the author addressed another memoir to the "Royal Society," accounting for the phenomenon, and shewing that it flowed from, and was confirmatory of his Theory.

The Theory of the Non-Decomposition of Water, and that Hydrogen was composed of Electricity and Water, was communicated to the Royal Society in a memoir forwarded from the continent to Dr. Roget, in April, 1840, and which was referred by the Council to the "Committee of Chemistry," who made no report thereon.

In December, 1841, the author addressed another memoir upon the same subject, to the Royal Society, accompanied with the dicta of Cavendish, Priestley, and Watt, as to Hydrogen being a compound body, which was also referred to the "Committee of Chemistry," but without any result.

CONSERVATISM in Science as well as in Legislation, is highly laudable up to a certain point: but when the system, or the tenets, are found to be unsatisfactory—to be utterly worthless in fact—it becomes a vice.

It will be seen from the above dates, that ultra-conservatism predominates in both the scientific *foci*—the "French Institute," and the "Royal Society."

The above enumeration, however, of papers, does not embrace one-half of those with which I *tormented* these two learned bodies upon the foregoing subjects. All to no purpose, *not one of the scientific and impartial committees to whom the papers were referred, would make a report;* so wary were these ephemeral professors of damaging their fragile wings. My only alternative was then to appeal to the public; but here again I have been compelled from time to time to fire off a fresh piece of artillery in order to rouse attention. But neither has this succeeded, as is evident from Mr. Grove's "GOLD MEDAL," and it is probable that but for the present publication, the TRUTHS that I have demonstrated would have sunk into oblivion.

INDEED WHAT ASSURANCE HAVE I EVEN NOW that indifference combined with scientific manœuvre, may not produce that result?

I have, however, the satisfaction that I shall not be constrained, like poor Galileo, to renounce these truths upon my knees, and whose only consolation at the conclusion of his humiliation, was to mutter, "*E pur se muove!*" Such was the lot of an enlightened Italian, when CARDINAL VIRTUES were in the plenitude of their power.

Sir David Brewster has stepped forward in defence of the scientific conservatism which thus treated that great philosopher.

Sir David says,* "Men are not necessarily obstinate *because they cleave to deeply rooted and venerable errors,*† nor are they absolutely dull when they are *long in understanding* and slow in embracing newly discovered truths." But he restores our spirits with a panacea.

"He who contends, (he says in a subsequent page) for truths which he has himself been permitted to discover, may well sustain the conflict in which presumption and error are destined to fall."

"The public tribunal may neither be sufficiently pure nor enlightened to decide upon the issue, *but he can appeal to posterity, and reckon with confidence on its sure decree.*"‡ AMEN!

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* See Sir David Brewster's Life of Galileo.

† How long have errors been held *venerable* by SAVANTS ?

‡ It seems never to have occurred to Sir David's penetration, that there is a possibility that in the *interim* between the author and POSTERITY, his truths may have gone to line some ignoble trunk, or been consigned to other more oblivious uses, and in which unfortunate case, this noble and generous posterity would be without the means of making him immortal.

FINIS.

LAUS DEO.

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